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BÉCHAMP OR PASTEUR?

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Pierre Jacques Antoine Béchamp

when Professor of Medical Chemistry and Pharmacy at the University of Montpellier 1857-1875

BÉCHAMP OR PASTEUR?

A Lost Chapter in the History of Biology

By

E. DOUGLAS HUME

Founded upon MS.
by

MONTAGUE R. LEVERSON, M.D. (Baltimore), M.A., Ph.D.

With a Foreword by

S. JUDD LEWIS, D.Sc., F.I.C.

"Truth will come to light"
—*Shakespeare*

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NOTE TO SECOND EDITION

Since the first edition of this book was sold out, two of its best friends, one in this country and one in America, have passed into the Great Beyond. Yet their influence stirs in this new edition, which has found other good friends to whom, for their help and encouragement, I tender grateful thanks.

Evidence of growing attention to Béchamp reaches us from all parts. In 1927, an account of him, written by Fr. Guermonprez, was published in Paris by Amédée Legrand, 93, Boulevard Saint-Germain. In the same year, on the 18th September, a bust of the great French scientist was unveiled at Bassing, his birthplace, before a distinguished gathering, when his genius and discoveries were loudly eulogised. News comes from New Zealand of successful medical work on the lines of Béchamp's teaching. In the United States of America, a text-book on Bacteriology is being written by Dr. Weiant, in collaboration with Dr. J. Robinson Verner, in which reference is to be made to *Béchamp or Pasteur?* and Béchamp's labours are to be recognised. From far away Mexico, a request comes from Dr. Hernàn Alpuche Solis to be allowed to undertake a Spanish translation of *Béchamp or Pasteur?* in order, as he puts it, "to publish the truth throughout the world."

Denials of the claims made for Béchamp's discoveries have been impossible; for, as Fr. Guermonprez writes, on page 18 of his *Béchamp: Études et Souvenirs:*—"To get a right idea of questions of priority, the works of Pasteur, Duclaux, or their pupils, are not the ones to study; but, instead, the impartial records of the learned Societies, particularly those of the Academy of Sciences of the Institute of France." There, in the cold type of the

printed word, the precedence of Béchamp's pronouncements to Pasteur's stands secure for good and all. Nevertheless, this personal side of the subject, in spite of its importance from the point of view of historical justice, is of less consequence than the results of building medical practice upon the insecure theoretical foundation described by Sir Almroth Wright as "the Pasteurian Decalogue." Of these commandments, he states, as reported in *The Times* of November 27th, 1931, "very few remain intact." On the other hand, there are increasing indications of modern medical views converging towards the microzymian doctrine. For instance, in *Health, Disease and Integration*, by H. P. Newsholme, M.A., M.D., F.R.C.P., B.Sc., P.D.H., a book published in 1929, on page 64, we find "the idea of a possible autonomous (self-produced) living enzyme or virus capable of giving rise to disease and capable of multiplication by reason of its living quality." The science of bio-chemistry, which occupies so wide a field to-day, is in no small measure an expansion of the teaching of Béchamp; while the remarkable results of X-Radiation lend support to his contention that in the microzymas (of the chromatinic threads) lies the secret of heredity. Reference may be made to the first of two articles by C. P. Haskins in the *General Electric Review* of July, 1932.

Of Béchamp, a story is related of how, when a tiny child, he was once caught telling a lie. His mother, on hearing of this on her return home in the evening, then and there turned her small son out of bed and, while whipping him soundly, impressed upon him her horror of falsehood. Béchamp, it is said, attributed his passionate regard for exactitude to this early lesson, which he never forgot. To all others, known and unknown, to whom Truth is precious, I am proud to dedicate the new edition of this book.

E. DOUGLAS HUME.

Woodford Wells.
October, 1932.

FOREWORD

The progress of natural science, like all other departments of knowledge, is associated with the personalities of its workers, and it often happens that the study of a man's life is the surest guide not only to the history of the science, but also to the discovery of neglected records made in days gone by. It is always a matter of absorbing interest to know how and by whom the foundations of natural truth, upon which we build our own more modern structures, were laid. We have long been accustomed to build on stones placed in position by the world-famed Pasteur, but it is not commonly recognized that many of these stones rest upon the deeper foundations laid by Pasteur's contemporary, Antoine Béchamp. It is fitting that one should hesitate to disturb stones set by those already gone from us, but when a substructure has once been revealed, there can be no question as to the liberty of extending the investigation. Probably no reader of this book will at first be prepared to accept much that is said in criticism of Pasteur and in worship of Béchamp, but as the perusal proceeds, his eyes will be opened to many references for which the author is in no way responsible except for their collation. It is greatly to be desired that the fundamental work of Béchamp should be far more widely recognized, and a debt is due to the author for throwing the limelight on his work.

S. JUDD LEWIS.

PREFACE

Many years ago, in New York, Dr. Montague R. Leverson chanced to come upon the writings of Pierre Jacques Antoine Béchamp. So greatly did he become imbued with the views of the French Professor, that he seized the first opportunity to travel to Paris for the purpose of making the latter's acquaintance. He was fortunate enough to arrive some months before the death of the great scientist and to receive from him in person an account of his discoveries and his criticisms of science, ancient and modern.

Henceforward it became the dearest wish of Dr. Leverson to place the case of Professor Béchamp, especially in regard to his relations with Pasteur, before the scientific world. Unable, owing to his great age, to carry out this project, the present writer, author of a short treatise on Béchamp, *Life's Primal Architects*, which originally appeared in *The Forum*, was pressed to undertake the work. Its aim is to arouse the interest of those more qualified to do justice to the memory of a genius, whose disadvantage it was to have lived far ahead of the scientific thought of his own day. For all deficiency in this presentment of his teachings, it is begged that the writer may be blamed and not the doctrines of the great teacher, to whose original works it is strongly urged that the reader should turn.

It only remains to mention those whose help has been of the greatest service. It is deeply to be regretted that the late Mr. R. A. Streatfeild, of the Department of Printed Books in the British Museum, is no longer here to receive the thanks so justly his due. These are most cordially rendered to Mr. L. H. E. Taylor, of the same Department, and to all the officials of the North Library for constant kindness and courtesy and for the facilities so generously afforded for research work. To M. Edouard Gasser, the son-in-law of Professor Béchamp, great indebtedness must

be expressed for particulars of the scientist's life and family. No words can adequately acknowledge the gratitude owed to Miss Lily Loat for unfailing assistance in regard to any point at issue, as well as for hours spent in proof-reading and in helping towards the preparation of the Index. The business arrangements in America and the acquirement of U.S.A. copyright could never have been accomplished without the very kind help of Mrs. Little and Mr. R. B. Pearson of Chicago, to whom warm thanks are extended. Last, but far from least, acknowledgment is gratefully made to the anonymous philanthropist whose generosity has brought about the publication of this book.

July, 1922. E. DOUGLAS HUME.

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INTRODUCTORY

CHAPTER I

Antoine Béchamp

At Villeneuve l'Étang, not far from Paris, on the 28th September, 1895, the death took place of a Frenchman who has been acclaimed as a rare luminary of science, a supreme benefactor of humanity. World-wide mourning, national honours, pompous funeral obsequies, lengthy newspaper articles, tributes public and private, attended the passing of Louis Pasteur. His life has been fully recorded; statues preserve his likeness; his name has been given to a system, and Institutes that follow his methods have sprung into being all over the world. Never has Dame Fortune been more prodigal with bounties than in the case of this chemist who, without ever being a doctor, dared nothing less than to profess to revolutionise medicine. According to his own dictum, the testimony of subsequent centuries delivers the true verdict upon a scientist, and adopting Pasteur's opinion as well as, in all humility, his audacity, we dare to take it upon ourselves to search that testimony.

What do we find?

Nothing less than a lost chapter in the history of biology, a chapter which it seems essential should be rediscovered and assigned to its proper place. For knowledge of it might tend, firstly, to alter the whole trend of modern medicine, and, secondly, to prove the outstanding French genius of the nineteenth century to have been actually another than Louis Pasteur!

For indeed this astonishing chapter denies the prevalent belief that Pasteur was the first to explain the mystery of fermentation, the cause of the diseases of silk-worms, and the cause of vinous fermentation; moreover, it shows that his theories of micro-organisms differed in basic essentials from those of the observer who seems to have been the real originator of the discoveries to which Pasteur has always laid claim. And so, since Truth is our object, we venture to ask for patient and impartial consideration of the facts that we bring forward in regard to the life-work of two French scientists, one of whom is barely known to the present generation, though much of its knowledge has been derived from him, while the name of the other has become a household word.

Twelve and a half years after the death of Pasteur, on the 15th April, 1908, there passed away in a modest dwelling in the student quarter of Paris an old man in his ninety-second year. His funeral was attended by a platoon of soldiers, for the nonogenarian, Professor Pierre Jacques Antoine Béchamp, had a right to this honour, as he had been a Chevalier of the Legion of Honour. Otherwise the quiet obsequies were attended only by the dead man's two daughters-in-law, several of his grandsons, a few of his old friends and an American admirer.[1] No pomp and circumstance in the last ceremonies indicated the passing of a great scientist, but, after all, it was far from the first time that a man's contemporaries had neglected his worth. Rather more than a century earlier, another Antoine, whose surname was Lavoisier, had been done to death by his countrymen, with the comment—"The Republic has no need of *savants!*" And now, with scant public notice, was laid in its last resting-place the body of perhaps an even greater scientist than the great Lavoisier, since this other Antoine, whose surname was Béchamp, seems to have been the first clear exponent of fermentative mysteries and the pioneer of authentic discovery in the realm of "the immeasurably small."

[1] Dr. Montague R. Leverson.

In the year in which he died, eight pages of the *Moniteur Scientifique* were required to set forth a list of his scientific works. The mere mention of his titles may suggest an idea of the stupendous labours of his long and arduous career. They were as follows:

Master of Pharmacy.
Doctor of Science.
Doctor of Medicine.

Professor of Medical Chemistry and Pharmacy at the Faculty of Medicine at Montpellier.

Fellow and Professor of Physics and of Toxicology at the Higher School of Pharmacy at Strasbourg and Professor of Chemistry of the same town.

Corresponding Member of the Imperial Academy of Medicine of France and of the Society of Pharmacy of Paris.

Member of the Agricultural Society of Hérault and of the Linnæan Society of the Department of Maine et Loire.

Gold Medallist of the Industrial Society of Mulhouse for the discovery of a cheap process for the manufacture of aniline and of many colours derived from this substance.

Silver Medallist of the Committee of Historic Works and of Learned Societies for works upon the production of wine.

Professor of Biological Chemistry and Dean of the Faculty of Medicine of Lille.

Honorary Titles

Officer of Public Instruction.
Chevalier of the Legion of Honour.
Commander of the Rose of Brazil.

Long though his life was, considerably outstretching the rather arbitrary limit of the Psalmist, it can only seem incredibly short when compared with a list of discoveries phenomenal for the life-span of one man. And as the history of the foundations of biology, as well as the work of Louis Pasteur, are both intricately connected with this extended career of usefulness, we will try to sketch a faint outline of the life-story of Pierre Jacques Antoine Béchamp.

He was born during the epoch that had just witnessed the finish of the Napoleonic wars, for it was on the 16th October, 1816, that he first saw light at Bassing in Lorraine, where his father owned a flour mill. The boy was

only eleven, when a change in his life occurred. His mother's brother, who held the post of French Consul at Bucharest, paid the Béchamps a visit and was struck by the intelligence and aptitude of young Antoine. He grew anxious to give him better opportunities than he would be likely to meet with in his quiet country home. We have not heard much of Antoine's mother; but when we find that his parents unselfishly allowed him, for his own good, to be taken away from them at the early age of eleven, we may be fairly certain that she was a clever, far-seeing woman, who might perhaps support Schopenhauer's theory that a man's mother is of more importance to him than his father in the transmission of brains! Be that as it may, when the uncle's visit ended, the small nephew went with him and the two undertook together the long and, in those days, very wearisome coach journey from Nancy to Bucharest.

It thus came about that Antoine saw much of the world and gained a thorough knowledge of a fresh language, advantages that strengthened and developed his alert intellect. Unfortunately, his kind relative died after a few years and the boy was left to face the battle of life alone. Friends came to his help and placed him as assistant to a chemist, who allowed him to attend classes at the University, where his brilliant genius made all learning easy and in 1833, without any difficulty, he obtained a diploma in pharmacy. In his youthful proficiency he presents a contrast to Pasteur, who, in his school days, was pronounced to be only an average pupil, and later, by an examiner, to be mediocre in chemistry.

Antoine was still under twenty when he returned to his native land and, after visiting his parents, started work at a chemist's in Strasbourg, which city at that time, with the rest of Alsace and Lorraine, formed part of France. His extraordinary powers of work were soon made manifest. Much of his spare time was devoted to the study of his own language, in which he acquired the polish of style that was to stand him in good stead in his future lectures and

literary labours. All the while, he continued his University course at the Academy of Strasbourg until he became qualified as a chemist. On obtaining his degree, he set up independently at Benfeld in Alsace, where he met and married Mlle. Clémentine Mertian, the daughter of a retired tobacco and beet-sugar merchant, who made him a capable wife. Science claimed so much of her husband's time that the training of their four children and the whole management of the household were left almost entirely to Mme. Béchamp.

Soon after the marriage, Antoine returned to Strasbourg to set up as a chemist; but this work did not nearly satisfy his vigorous energy and he now prepared himself to occupy a Professor's Chair. He soon realised his aim. In a short time he acquired the diplomas of Bachelor of Science and Letters and of Doctor of Medicine and was nominated Professor at the School of Pharmacy in the Faculty of Science, where for a time he took the place of his colleague, Pasteur.

These notable rivals both worked in the full flush of early enthusiasm in the capital of Alsace. But a difference already marked their methods. Pasteur seems never to have left an effort of his unrecorded; every idea as to the tartaric and racemic acids, about which he was then busied, appears to have been confided to others; letters detailed his endeavours; his invaluable patron, the scientist, Biot, was especially taken into his confidence, while his approaching honour and glory were never allowed to absent themselves from his friends' minds. He wrote to Chappuis that, on account of his hard work, he was "often scolded by Mme. Pasteur, but I console her by telling her that I shall lead her to fame."[1]

From the start, Antoine Béchamp was utterly indifferent to personal ambition. Never of a pushing temperament, he made no effort to seek out influential acquaintances and advertise his successes to them. Self-oblivious, he was entirely concentrated upon nature and its mysteries, never

[1] *The Life of Pasteur*, by René Vallery-Radot, p. 58 (Pop. Ed.).

resting till something of these should be revealed. Self-glorification never occurred to him and while the doings of Pasteur were being made public property, Béchamp shut in his quiet laboratory, was immersed in discoveries, which were simply published later in scientific records without being heralded by self-advertisement.

The work that he accomplished at Strasbourg was prolific in benefits for France in particular and for the world at large. It was there that his studies led him to the discovery of a new and cheap method of producing aniline, which up to 1854 had been so costly as to be useless for commercial purposes. The German chemist, August Wilhelm von Hofmann, who for many years carried on work in England, after investigating the results of earlier discoveries, produced aniline by subjecting a mixture of nitro-benzene and alcohol to the reducing action of hydrochloric acid and zinc. Béchamp, in 1852, showed that the use of alcohol was unnecessary and that zinc could be replaced by iron filings, also that either acetic or hydrochloric acid may be used.[1] By thus simplifying and cheapening the process, he conferred an enormous benefit on chemical industry, for the cost of aniline fell at once to 20 francs and later to 15 francs a kilogramme; while, moreover, his invention has continued in use to the present day: it is still the foundation of the modern method of manufacture in the great aniline dye industry, which has been all too much appropriated by Germany. The *Maison Renard*, of Lyons, hearing of Béchamp's discovery, applied to him and with his help succeeded in a cheap production of fuchsin, otherwise magenta, and its varieties. The only return made to Béchamp, however, was the award, ten years or so later, of a gold medal from the Industrial Society of Mulhouse. Neither does any recognition seem to have been made to him for his discovery of a compound of arsenic acid and aniline, which, under the name of atoxyl, is used in the treatment of skin diseases and of sleeping sickness.

[1] Confirmed in Richter's *Organic Chemistry* and in Thorpe's *Dictionary of Applied Chemistry* (1921).

Another work of his that was to prove especially prolific in results was his application of polarimetric measurements to his observations on the soluble ferments. The polarimeter, that instrument in which light is polarised or made to vibrate in one plane by means of one Nicol prism and examined by means of a second Nicol prism, was utilised by him in experiments, the general results of which were that he was enabled before any other worker to define and isolate a number of ferments to which he was also the first to give the name of *zymases*. In dealing with this work later on, we shall show how his discovery, even to its nomenclature, has been attributed to somebody else.[1]

So interminable were Béchamp's labours, so numerous his discoveries, that it is hard to know which to single out. He studied the monobasic acids and their ethers and invented a method of preparing the chlorides of acid radicles be means of the derivatives of phosphorous. He made researches upon lignin, the characteristic constituent of the cell-walls of wood-cells, and showed clearly the difference between the substituted organic nitro-compounds, like ethyl nitrite and the nitro-paraffins. As we shall see subsequently, he was the first really to establish the occurrence in, and distribution by, the atmosphere of micro-organisms, such as yeast, and to explain the direct agent in fermentation to be the soluble ferment secreted by the cells of yeast and other such moulds. Cleverest of chemists and microscopists, he was also a naturalist and a doctor, and gradually his chemical work led him on to his astonishing biological discoveries. The explanation of the formation of urea by the oxidation of albuminoid matters and his clear demonstrations of the specificity of the latter formed only part of the strenuous labours that led to *his discovery that the "molecular granulations" of the cells assist in fermentation, that they are autonomous entities, the living principle, vegetable and animal, the originators of bodily processes, the factors of pathological conditions, the agents of decomposition, while, incidentally, he believed them to be capable of evolving into bacteria.*

[1] See pp. 74, 75, 162.

These conclusions may not all yet be adopted, but as so many of Béchamp's other teachings have come, by the independent work of some and the plagiarisms of others, to be generally accepted, it would seem, to say the least of it, possible that his amazing revelation of nature's biological processes may become public property and we wish to ensure the recognition of its legitimate parentage.

He showed that the cell must no longer be regarded in accordance with Virchow's view as the unit of life, since it is built up by the cell-granules within it. He it was, it seems, who first drew attention to the union of these same cell-granules, which he called "microzymas," and to the rod-like groupings that result, which now go by the name of chromosomes. He laid great stress upon the immeasurable minuteness of his microzymas and from his teaching we can well infer his agreement in the belief that myriads must be ultra-microscopic, although he had far too exact a mind to descant in modern airy fashion upon matters that are purely conjectural. Where he exhibited his practical genius was that, instead of drawing fancy pictures of primeval developments of chromatin, he traced by rigid experiment the actual building up of cells from the "molecular granulations," that is, microsomes, or microzymas. It was never his method to draw conclusions except from a sure experimental basis.

It was while Béchamp was undertaking his researches upon fermentation, at the very time that he was engaged upon what will prove to be part of what he named his "Beacon Experiment," that he was called from Strasbourg to Montpellier to occupy the Chair of Medical Chemistry and Pharmacy at that famous University.

The period that followed seems likely to have been the happiest of his life. Filling an important position, he carried out his duties with the utmost distinction, his demonstrations before students gaining great renown. He had already made and was further developing extraordinary discoveries, which were arresting attention both in and beyond France. These gained him the devoted friendship

of his admirer and future collaborator, Professor Estor, a physiologist and histologist, who combined the duties of physician and surgeon at the Montpellier Hospital. Béchamp, also, had the advantage of medical training, and though he never practised as a doctor, his pathological studies were continuous and he was daily in touch with the work of physicians and surgeons, such as Courty, besides Estor, and himself took full advantage of the experience to be obtained in hospital wards. His and Estor's more theoretical studies were checked and enlarged by their intimacy with the vast experiments that Nature carries out in disease. Both were men accustomed to the strictness of the experimental methods of Lavoisier and their clinical and laboratory work moved side by side, the one confirming and establishing the other.

Without ever neglecting his professorial duties, sufficiently arduous to absorb the whole time of an ordinary mortal, Béchamp yet laboured incessantly, both by himself and with Professor Estor, at the problems that his researches were developing. A little band of pupils gathered about them, helping them, while far into the night constantly worked the two enthusiasts, often, as Béchamp tells us,[1] quite awestruck by the wonderful confirmation of their ideas and verification of their theories. Such toil could only be continued by one possessed of Professor Béchamp's exuberant health and vitality, and it possibly told upon Professor Estor, whose early death was attributed partly to his disappointment that the popular germ-theory of disease, in all its crudity, should have seized public attention instead of the great microzymian doctrine of the building up of all organised matter from the microzymas, or "molecular granulations" of cells.

His incessant work, which kept him much apart from his family, was the only hindrance to Béchamp's enjoyment of a happy domestic life. An excellent husband and father, he was always thoughtful for others, and in all his dealings was as kind as he was firm. His lectures were

[1] *La Théorie du Microzyma, par A. Béchamp*, p. 123.

made delightful by his easy eloquence and perfect enunciation, no less than by the clearness of his reasoning; while his social manner possessed the grace and courtliness that are typical of the polished inhabitants of *la belle France*. Well above medium height, his clear eye and ruddy complexion gave unstinted proof of the perfect sanity of mind and body that he was blessed with throughout the whole course of his long life. His powerful forehead testified to the strength of his intellect, while his nose was of the large aquiline type that so usually accompanies creative force and energy. His hair was brown and his forceful eyebrows were strongly marked above the large eyes of an idealist, a dreamer of dreams, which in his case were so often realised.

To the physiognomist, a comparison of the looks of the rivals, Béchamp and Pasteur, gives a key to their respective scientific attitudes. Alert determination is the chief characteristic of Pasteur's features; intellectual idealism of Béchamp's. Pasteur approached science from the commercial, that is to say, the utilitarian standpoint, no less self-advantageous because professedly to benefit the world. Béchamp had ever the artist's outlook. His thirst was for knowledge, independent of profit; his longing to penetrate the unexplored realm of Nature's secrets; the outer world was forgotten, while, pace by pace, he followed in the footsteps of truth. It never occurred to him to indite compliments to influential acquaintances and announce at the same time the dawning of a new idea. The lessons he learned in his quests he duly noted and communicated to the French Academy of Science and at first ignored the fact that his observations were pirated. When finally his silence changed to protest, we shall see, as we proceed, that his patience had been stretched to snapping point. Himself so exact in his recognition of every crumb of knowledge owed to another, he could only feel contempt for pilferers of other men's ideas, while his exuberant vigour and energy fired him with uncompromising opposition to those who, not content with reaping where he had

sown, trampled with their distortions upon a harvest that might have been so abundant in results.

It was during the years spent at Montpellier that his open rupture came with Pasteur, on account, as we shall see farther on, of the latter's appropriation of Béchamp's explanation of the causes of the two diseases that were then devastating silk-worms and ruining the French silk industry. Though there was no escaping the fact that Pasteur's opinions on the subject had been erroneous until Béchamp had provided the proper solution, no voices were raised in condemnation of the former's methods. He had already gained the ear of the public and acquired Imperial patronage. In all ages, the man of influence is a hard one to cross swords with, as Béchamp was to find.

But at Montpellier he had not yet drained the cup of life's bitterness. Hope still swelled high for the future, especially when, as time passed, a new assistant rose up, and Béchamp's elder son, Joseph, became a sharer in his work. This young man, whose lovable character made him a general favourite, took at an early age his degree in science, including chemistry, besides qualifying as a doctor of medicine. It seemed certain that he would some day succeed his father at the University.

But for France a sad day was dawning and for Béchamp a disastrous change in his career. 1870 came with the descent of the Prussians and the humiliation of the fair land of France. Those districts of Alsace and Lorraine, the home of Béchamp's young boyhood and early manhood, were torn away, their populace left lamenting:—"Though our speech may be German, our hearts are French!" France, stricken, was far from crushed. A longing stirred to show that, though despoiled of territory, she could yet dominate in the world of thought. So it came about that, as an intellectual stimulus, Universities were founded in different places under ecclesiastical patronage. It was hoped that the Church of Rome might hold sway over mental activities. Lille was one of such centres, and about the year 1874, Béchamp was importuned to take the post

there of Dean of the Free Faculty of Medicine. Some wise friends advised him not to leave Montpellier; but, on the other side, he was bombarded with entreaties to take up work at Lille. Finally, and entirely from patriotic motives, he allowed himself to be persuaded to leave his dear University of Montpellier, teeming with happy memories of successful work. His altruistic wish to benefit at one and the same time France and science brought about his acquiescence in the change. He moved to the North with his son Joseph, the latter having been appointed Professor of Toxicology at Lille.

All might have gone well had it not been for the clerical Directors of the house of learning, whose want of faith was well advertised by their intellectual timidity. Like all who fence in belief with dogma—and religious priesthoods are by no means the only builders of such enclosures—the anxious ecclesiastics were determined to set boundaries to science and keep thought within barriers. The inevitable result was continual friction between the clerical Directors and the lay Professors of the University.

Unfortunately, Béchamp entered this unpropitious atmosphere just at the moment when he was putting the finishing touches to his exposition of the microzymas, that is, the infinitesimal cellular granules, now known as microsomes, which he considered to be the formative agents of the cells which compose all animal and vegetable forms. This stupendous conception of the processes of Creation at once raised a note of protest from the narrow-minded clerics. Here was a man who dared to profess to describe Nature's methods, instead of complacently resigning them to mystery.

Pasteur seems never to have fallen foul of the ecclesiastics; partly, perhaps, because he did not come into the same close contact; but more probably because, with his worldly wisdom, he was content to profess leadership in science and discipleship in religion; besides, had he not also gained the patronage of the great? Béchamp's deep insight had taught him the connection between science

and religion, the one a search after truth, and the other, the effort to live up to individual belief. His faith had widened to a breadth incomprehensible to dogmatic bigots, so that even the appointment of a Commission was suggested to recommend the placing on the Roman *Index* of his book *Les Microzymas*, which culminates in the acclamation of GOD as the Supreme Source. Béchamp's teachings are in direct opposition to materialistic views. But those priests had not the insight to see that the Creator is best demonstrated by the marvels of Creation, or appreciate the truth taught by Ananias, Azarias and Misael in calling upon the Lord to be praised through His Works!

Impatient of petty bickerings, like most men of large intellect, Béchamp found himself more and more at a disadvantage in surroundings where he was misinterpreted and misunderstood. Neither were these his only worries. He was suffering from the jealousy he had inspired in Pasteur, and was smarting from the latter's public attack upon him at the International Medical Congress in London, which they had both attended in the year 1881. Such behaviour on the part of a compatriot before a foreign audience had seared the sensitive spirit of Béchamp and decided him to reply to Pasteur's plagiarisms. As he writes in the Preface to *Les Microzymas*[1]:— "The hour to speak has come!"

Another hour was soon to strike for him. After enduring for about eleven years the prejudices and persecutions of the Bishops and Rectors of Lille, he felt unable to continue to submit to the restraints placed upon his work. No cause of complaint could be upheld against him; the charge of materialism in his views could not be supported; but rather than have his life-work continually hampered, the Professor regretfully decided to send in his resignation, and his son Joseph, for his father's sake, felt impelled to do the same. Thus father and son, the shining lights of Lille's educational circle, found their official careers cut short and experienced that bitterness of spirit understood only by those whose chief lode-star has been their work.

[1] p. 8.

The younger Béchamp during his stay at Lille had married a Mlle. Josephine Lang from Havre, and, owing to this new connection, the Béchamp family moved to the seaboard town and set up in business as chemists. A scientific laboratory enabled the two strenuous workers to undertake medical analyses and continue their research.

But, again, the hand of Fate dealt heavily with Antoine Béchamp. His son Joseph, well known as a clever chemist, was constantly employed in making chemical assays, which work occasionally took him out to sea. On one of these expeditions he caught a severe chill: double pneumonia set in, and in a few days ended his comparatively short and most promising life of forty-four years.

It was Antoine Béchamp's sad lot to outlive his wife and his four children. Quite against his wish, his younger daughter had been persuaded into taking the veil, and conventual severities brought about her death at an early age. His elder daughter had married at Montpellier in 1872, M. Edouard Gasser, who owned vineyards at Remigny, and left five children, one daughter and four sons, one of whom was at an early age carried off by typhus, while the other three lived to do service for France in the Great War.

Joseph Béchamp left six children, four daughters and two sons, one of whom died young. The other had no taste for science and disposed of his father's pharmacy and laboratory. He died a bachelor in 1915.

Antoine Béchamp's younger son, Donat, who died in 1902, married a Mlle. Marguerite Delarue, and left three sons, the two younger of whom were destined to lay down their lives in the Great War. The eldest, then a doctor in the Russian Army, narrowly escaped death by drowning through the sinking of the hospital ship "Portugal" by a German submarine. Sole living male representative of his grandfather, he is said to inherit the same genius. Without the least effort, he has taken diplomas in medicine, chemistry and microscopy, and with the same facility has

qualified in music and drawing, the arts being as easy to him as the sciences.

We will now return to Antoine Béchamp at the point where we left him at Havre, suddenly bereft of the gifted son on whom not only his family affections, but his scientific hopes were placed. Antoine Béchamp was indeed experiencing the rigorous discipline of which the Chinese philosopher, Mencius, thus speaks:—"When Heaven demands of a man a great work in this world, it makes his heart ache, his muscles weary, his stomach void and his mind disappointed; for these experiences expand his heart to love the whole world and strengthen his will to battle on where others fall by the way."

Havre had become a place of sorrowful memories, and Professor Béchamp was glad to move to Paris. Here he could continue his biological work in the laboratory of the Sorbonne, generously put at his disposal by his old colleague, M. Friedel, who, with another old friend, M. Fremy, had never ceased to deplore his patriotic unselfishness in abandoning his great work at Montpellier. Up to 1899, that is to say, until he was eighty-three years of age, this grand old man of science never ceased his daily labours in the laboratory. After that time, though no longer able to continue these, he worked no less diligently to within a few days of his death, collecting and arranging the literary results of his long years of toil, while he continued to follow and criticise the course of modern science. Up to the very end his brilliant intellect was undimmed. Patriarchal in dignity, he was always ready to discuss old and new theories and explain his own scientific ideas. Though sorrow and disappointment had robbed him of his natural cheerfulness, he was in no sense embittered by the want of popular recognition. He felt that his work would stand the test of investigation, that gradually his teaching would be proved true and that the verdict of coming centuries could not fail to raise him to his proper place. Even more indifferent was he to the lack of riches. For him, labour was its own reward and success dependent

upon the value of the results of work and not upon pecuniary profit, which as often as not falls to the share of plagiarists, at the expense of men of real worth.

And so, in 1908, came the April day when, worn out by labour, Antoine Béchamp could no more rise from the bed in his room where, on the walls, four crucifixes testified to self-sacrifice as the ladder by which mankind scales upwards. His belief was proved, to quote his own words,[1] in Him, "whom the founders of science, the greatest geniuses that are honoured by humanity from Moses to our own day, have named by the name—GOD!" "My faith!" was one of his last whispered utterances, as his life ebbed away, and of faith he was well qualified to speak, he who had delved so deeply into nature's marvels and the mysteries of the invisible world! Calm and confident to the end, his trust was immovable. Well does the *Moniteur Scientifique* prophesy that time will do justice to his discoveries and that, the living actors once passed from the stage and impartial judgment brought into play, Béchamp's genius will be revealed to the world.

He taught that which was marvellous and complex, like all nature's workings, and public ignorance eagerly snatched instead at what was simple and crude. But error, having the canker of destruction within itself, falls to pieces by degrees. Already the need arises for a saner solution of disease than the mere onslaughts of venomous microbes and a fuller explanation of the processes of biological upbuilding and disruption, of life and death. And to whom could the world go better than, as we shall see, to the inspirer of what was correct in Pasteur's teaching, the true revealer of the mystery of fermentation, the exponent of the rôle of invisible organisms, the chemist, naturalist, biologist and physician, Professor Pierre Jacques Antoine Béchamp?

[1] *Les Microzymas, par A. Béchamp*, p. 926.

PART ONE

THE MYSTERY OF FERMENTATION

CHAPTER II

A Babel of Theories

Before starting upon any examination of Béchamp's and Pasteur's contributions to the scientific problems of their age, it may be well to revert to the utter confusion of ideas then reigning in the scientific world in regard to the mysteries of life and death and the phenomenon of fermentation. The ensuing chapter can only hope to make clear the utter absence of clarity in regard to these leading questions; and though the work of earlier scientists invariably led up to subsequent discovery, yet in the days when Antoine Béchamp and Louis Pasteur commenced their life-work, the understanding of the subject was, as we shall see, in a state of confusion worse confounded.

Three paramount problems then faced the scientific inquirer.

1. What is living matter, this protoplasm, so-called from Greek words meaning "first" and "formed"? Is it a mere chemical compound?

2. How does it come into being? Can it arise spontaneously; or is it always derived from pre-existing life?

3. What causes matter to undergo the change known as "fermentation"?

Among Professor Béchamp's prolific writings quite a history may be found of the confused babel of theories on these subjects.

To start with the first query: there was merely the vague explanation that protoplasm is the living matter from which all kinds of living beings are formed and to the properties of which all are ultimately referred. There

was belief in a substance called albumen, best represented by white of egg, which was said to mix with certain mineral and other matters without changing its nature. J. B. Dumas demonstrated that such "albuminoids" comprise not one specific thing, but many different bodies; but the contrary opinion prevailed, and for such substances "protoplasm" was adopted as a convenient term. It was "the physical basis of life," according to Huxley; but this hardly illumined the difficulty, for thus to pronounce protoplasm to be matter living *per se*, was not to explain the mystery of how it was so, or its origin and composition. True, Huxley further declared all living matter more or less to resemble albumen, or white of egg; but this latter was also not understood either by biologists or chemists. Charles Robin regarded it as being of the type of the mucoids, that is to say, as resembling mucus, which latter was so shrouded in mystery that Oken called it *Urschleim* (primordial slime), and the botanist, Hugo Mohl, identified it with protoplasm, thus dignifying mucus as the physical basis of all things living!

Claude Bernard tried to determine the relation of protoplasm to organisation and life and combated the general idea that every living body must be morphologically constituted, that is to say, have some structural formation. He argued that protoplasm gave the lie to this belief by its own structural indefiniteness. Charles Robin followed the same view and gave the name of "*Blastème*," from a Greek word, meaning to sprout, to the supposed primordial source of living forms.

This was nothing but the old idea of living matter, whether called protoplasm or *blastème*. A cell, a fibre, a tissue, any anatomical element was regarded as living simply because of its formation by this primordial substance. Organisation was said to be its "most excellent modification." In short, formless matter was supposed to be the source of all organised living forms. In a kind of despair of any experimental demonstration of organisation and life, a name was invented for a hypothetical substance

magically alive, although structurally deficient. Imagination played more part in such a theory than deduction from tangible evidence. Thus we find that the physician, Bichat, who made a name for himself in science before he died in 1802, at the early age of 31, could not accept such an explanation and declared the living parts of a living being to be the organs formed of the tissues.

A great step was gained when Virchow thought he saw the cell in the process of being built up, that is, structured, and thus jumped to the conclusion that it is self-existent and the unit of life, from which proceed all organised forms of developed beings.

But here a difficulty arose, for the cell proved as transitory as any other anatomical element. Thus many scientists returned to the belief in primordial structureless matter, and opinion oscillated between the views held by cellularists and protoplasmists, as the opposing factions were designated. Utter confusion reigned among the conflicting theories which struggled to explain how a purely chemical compound, or mixture of such compounds, could be regarded as living, and all sorts of powers of modification and transformation were ascribed to it, with which we need not concern ourselves.

Instead, let us consider the second problem that faced Béchamp and Pasteur when they started work, namely, whether this mysterious living substance, which went by so many names, could arise independently, or whether pre-existing life is always responsible. It is hard to realise, nowadays, the heated controversy that raged in the past around this perplexing mystery. The opposing camps of thought were mainly divided into the followers of two eighteenth-century priests, Needham, who claimed that heat was sufficient to produce animalculæ from putrescible matter, and Spallanzani, who denied their appearance in hermetically sealed vessels. The first were named Sponteparists from their belief that organised life is in a constant state of emergence from chemical sources, while the second were named Panspermists from their theory of

a general diffusion of germs of life, originally brought into being at some primeval epoch.

For the latter view, the teaching of Bonnet, following upon that of Buffon, was chiefly responsible; while Buffon's ideas are reminiscent of the ancient system ascribed to Anaxagoras. According to this last, the universe was believed to be formed of various elements, as numerous as its different substances. Gold was supposed to be formed of particles of gold, a muscle, a bone, a heart, to be formed of particles of muscle, of bone, of heart. Buffon taught that a grain of sea-salt is a cube composed of an infinite number of other cubes, and that there can be no doubt that the primary constituent parts of this salt are also cubes, which are beyond the powers of our eyes and even of our imagination.

This was an experimental fact, says Béchamp,[1] and was the basis of the system of crystallography of Hauy.

Buffon argued in the same strain that "in like manner that we see a cube of sea-salt to be composed of other cubes, so we see that an elm is but a composite of other little elms."

Bonnet's ideas[2] were somewhat similar; the central theme of his teaching being the universal diffusion of living germs "capable of development only when they meet with suitable matrices or bodies of the same species fitted to hold them, to cherish them and make them sprout —it is the dissemination or panspermy that, in sowing germs on all sides, makes of the air, the water, the earth, and all solid bodies, vast and numerous magazines where Nature has deposited her chief riches." He maintained that "the prodigous smallness of the germs prevents them from being attacked by the causes that bring about the dissolution of the mixtures. They enter into the interior of plants and of animals, they even become component parts of them, and when these composites undergo the law of

[1] *Les Microzymas*, p. 30.

[2] See *Ire partie*; *Oeuvres d'Histoire Naturelle de Bonnet*; V. pp. 83-86. Neuchâtel, 1779.

dissolution, they issue from them unchanged to float in the air, or in water, or to enter into other organised bodies."

Such was the imaginative teaching with which Bonnet combated the doctrine of spontaneous generation. When it came to practical experimental proof, one party professed to demonstrate the origin of living organisms from putrescible matter in sealed vessels; the other party denied any such possibility if air were rigorously excluded; while a pastry-cook, named Appert, put this latter belief to a very practical use and started to preserve fruits and other edibles by this method.

And here we are led to the third conundrum—What causes matter to undergo the change known as fermentation?

It is a puzzle that must have been brought home to many a housewife ignorant of scientific problems. Why should the milk left in the larder at night have turned sour by the morning? Such changes, including the putrefaction that takes place after the death of an organism, were so much of a mystery that the causes were considered occult for a long time. Newton had discoursed of the effect being due to an origin of the same order as catalysis—a process in which a substance, called a catalytic agent, assists in a chemical reaction but is itself unchanged. The myriads of minute organisms revealed, later on, by the microscope in fermenting and putrefying matters, were at first believed to be mere results of the general process of putrefaction and fermentation.

A new idea was introduced by Cagniard de Latour, who maintained that fermentation is an effect accompanying the growth of the ferment. That is to say, he looked upon the ferment as something living and organised, by which fermentation is rendered a vital act. It was the microscopic study of beer-yeast, undertaken about the year 1836, which brought him to the opinion that the oval cells he observed were really alive during the production of beer, decomposing sugar into carbonic acid and alcohol. Turpin, the botanist, interpreted this as meaning that the

globule of yeast decomposes sugar in the act of nourishing itself. J. B. Dumas maintained the necessity for nitrogenised albuminoid matter, as well as sugar, for food for yeast cells. Schwann, the German, went furthest of all by declaring that all fermentation is induced by living organisms and undertook experiments to prove these to be air-borne. But, in spite of other experiments confirming Schwann's work, for a time this teaching was set aside for the view that vegetable and animal matters are able to alter of themselves. For instance, the theory was held that by dissolving cane-sugar in water it changes of itself into grape-sugar, or glucose; or, using technical terms, cane-sugar undergoes inversion spontaneously.[1]

Such, roughly speaking, were scientific ideas at the middle of the nineteenth century, when Antoine Béchamp and Louis Pasteur appeared on the scene with details of their respective experiments. As Pasteur is renowned as the first to have made clear the phenomenon of fermentation, besides being appraised as the one who overthrew the theory of spontaneous generation, let us, instead of taking this on trust, turn to the old French scientific documents and see for ourselves what he had to say in the year 1857.

[1] The usual product of this hydrolysis, or inversion of cane-sugar, is invert-sugar; but, as this was formerly described as grape-sugar, that expression is usually retained here.

CHAPTER III

PASTEUR'S MEMOIRS OF 1857

LOUIS PASTEUR, the son of a tanner, was born at Dôle in the year 1822. Intense strength of will, acute worldly wisdom and unflagging ambition were the prominent traits of his character. He first came into notice in connection with crystallography by discovering that the crystalline forms of the tartrates are hemihedral. His son-in-law has recorded his jubilation over his early achievement and has told us how he left his experiment to rush out of the laboratory, fall upon the neck of a Curator, whom he met accidentally, and then and there drag the astonished man into the Luxembourg garden to explain his discovery.[1]

Work so well advertised did not fail to become a topic of conversation and eventually reached the ears of M. Biot. On hearing of this, Pasteur wrote at once to ask for an interview with this well-known scientist, with whom he had no previous acquaintance, but upon whom he now showered every attention likely to be appreciated by the rather misanthropical old worker, whose influential patronage became undoubtedly the first contributory factor in the triumphal career of the ambitious young chemist. All the same, M. Biot's persuasions never succeeded in gaining Pasteur a place in the Academy of Science. This he only obtained after the former's death, when nominated by the Mineralogical Section, and then oddly enough, exception began to be taken at once to his early conclusions on crystallography.[2]

This, however, was not until the end of 1862. Meanwhile, in 1854, Pasteur was appointed Professor and Dean

[1] *The Life of Pasteur*, by René Vallery-Radot, pop. ed. p. 39.
[2] *The Life of Pasteur*, by René Vallery-Radot, pp. 101, 102.

of the new Faculty of Science at Lille. In 1856, a request for advice from a local manufacturer of beetroot alcohol made him turn his attention to the problem of fermentation, which was then exercising the minds of the learned. His observations were interrupted by a journey to Paris to canvass for votes for his election to the Academy of Science. Obtaining only sixteen and completely failing in his attempt to enter the select circle of Academicians, Pasteur returned to Lille to his study of fermentations.

In spite of the work done by Cagniard de Latour, Schwann and others, the idea was prevalent that animal and vegetable matters are able to alter spontaneously, while the authority of the famous German chemist, Liebig, carried weight when he asserted that yeast induces fermentation by virtue of progressive alteration in water in contact with air.[1] Another German, named Lüdersdorff, so we learn from Béchamp,[2] had undertaken experiments to prove that yeast ferments sugar because it is living and organised. An account had been published in the Fourth Volume of the *Traité de Chimie Organique*, which appeared in 1856.

Now let us examine Pasteur's contribution towards this subject the following year, since at that date popular teaching assigns to him a thorough explanation of fermentation.

During 1857 Pasteur left Lille to work at the *École Normale* in Paris; but we are not here concerned with his movements, but simply with what he had to reveal on the mysterious subject of fermentation.

His son-in-law tells us[3] that it was in August, 1857, that, after experimenting in particular with sour milk, Pasteur first made a Communication on "Lactic Fermentation" to the Scientific Society of Lille. Be this as it may, we find his extract from a Memoir on the subject in the *Comptes Rendus* of the French Academy of Science, Nov. 30, 1857.[4] The

[1] *Traité de Chimie Organique, traduit par Ch. Gerhardt,* Introduction, p. 27. 1840.
[2] *Les Grands Problèmes Médicaux, par A. Béchamp,* p. 62.
[3] *The Life of Pasteur,* p. 83.
[4] *Comptes Rendus* 45, p. 913. *Mémoire sur la fermentation appelée lactique.*

entire Memoir was printed in April, 1858, in the *Annales de Chimie et de Physique*[1] and from this latter we gain full details.

The experiment consisted in Pasteur taking the substance developed in ordinary fermentation, nourished by sugar, chalk, casein or fibrin, and gluten (an organic matter occurring in cereals) and placing it in yeast broth (a complex solution of albuminoid and mineral matters), in which he had dissolved some sugar and added some chalk.

There was nothing new in the procedure, so Béchamp points out[2]; it was only the same experiment that Liebig had undertaken some sixteen or seventeen years previously. Unlike Liebig, he did not ignore microscopic examination and so made observations that had been missed by the German chemist. Thus Pasteur is able to tell us that a lactic ferment is obtained, which, under the microscope, has the appearance of little globules, which he named "lactic-yeast," no doubt from their resemblance to yeast, although in this case the little globules are much smaller. In short, he saw the minute organism known today to be the cause of lactic-acid fermentation.

Now let us go on to his remarkable explanation of the phenomenon. He tells us that it is not necessary to introduce the lactic ferment in order to prepare it, as "*it takes birth spontaneously* as easily as beer-yeast every time that the conditions are favourable."[3] This assertion surely demonstrates Pasteur's belief in the spontaneous generation both of beer-yeast and of that which he called "lactic-yeast." It remains to be seen what "the favourable conditions" are, according to his teaching. He tells us before long. "These globules of lactic-yeast take birth spontaneously in the body of the albuminoid liquid furnished by the soluble part of the (beer) yeast."[4] There is certainly nothing in

[1] *A. de Ch. et de Ph., 3e série*, 52, p. 404.

[2] *Les Grands Problèmes Médicaux*, p. 56 *et suivant.*

[3] "*elle prend naissance spontanément avec autant de facilité que la levûre de bière toutes les fois que les conditions sont favorables.*" *A. de Ch. et de Ph. 3e série*, 52, p. 413.

[4] "*Les globules prennent naissance spontanément au sein du liquide albuminoid fourni par la partie soluble de la levûre.*" *A. de Ch. et de Ph. 3e série*, 52, p. 415.

this to overthrow the general belief in spontaneous generation. But, in fairness, we must not overlook a note that he added to the full edition of his Memoir, as we find it in the *Annales de Chimie et de Physique.*[1] Before this account appeared in April, 1858, Professor Béchamp, as we shall find, had provided the French Academy of Science with an illuminating explanation of the origin of ferments. In face of Béchamp's irrefutable views, Pasteur may have thought it only wise to add a proviso to a Memoir that from start to finish has no solution whatever to offer as to the appearance of moulds except a spontaneous origin. Therefore, by the sentence "it (lactic-yeast) takes birth spontaneously as easily as beer-yeast," we see a star and looking below find a foot-note, in which he says he uses the word "spontaneously" as "the expression of a fact," but reserves the question of spontaneous generation.[2] Certainly any denial of it is completely excluded from this Memoir with its assertion of the spontaneous appearance of beer-yeast and "lactic-yeast." Where Pasteur differed from other Sponteparists was in omitting to attempt any explanation of such a marvel.

His followers, ignoring the confusion of his views, have seized upon the concluding statement in this same memoir as a triumphant vindication of the correctness of his teaching, since he said:—"Fermentation shows itself to be correlative of life, of the organisation of globules, not of the death and putrefaction of these globules, still more that it does not appear as a phenomenon of contact."[3] But this was only what others had said and had gone some way to prove years before him. So devoid was he of proof that he had to make the following admission in regard to his hypothesis that "the new yeast is organised, that it is a living being," namely:—"If anyone tells me that in these conclusions I am going beyond facts, I reply that this is

[1] *A. de Ch. et de Ph. 3e série*, 52, p. 413.

[2] "*Je me sers de ce mot comme expression du fait, en réservant complêtement la question de la génération spontanée.*"

[3] *ibid.* p. 418.

true, in the sense that I frankly associate myself with an order of ideas[1] that, to speak correctly, cannot be irrefutably demonstrated."

We have, therefore, in Pasteur's own words, his confession of non-comprehension of a problem that the rigid experiments of another worker, Professor Béchamp, had already, as we shall shortly see, solved by an irrefutable demonstration. The reason why Pasteur should get the credit for demonstrating that which he owned he could not demonstrate is as much of a puzzle to the lover of historical accuracy as was the phenomenon of fermentation to Pasteur.

However, let us not deny ourselves a thorough examination of his work, and now consider his *Memoir upon Alcoholic Fermentation*, of which his son-in-law, M. Vallery-Radot, tells us[2] that Pasteur said "The results of these labours (on lactic and alcoholic fermentation) should be put on the same lines, for they explain and complete each other."

We find the author's extract from this latter Memoir among the Reports of the French Academy of Science of the 21st. Dec., 1857.[3]

Pasteur's procedure in this experiment was as follows:—He took two equal quantities of fresh yeast, washed in water. One was left to ferment with pure sugared water, and after having extracted from the other all its soluble part by boiling it with plenty of water and filtering it to get rid of the globules, he added to the limpid liquor as much sugar as he used in the first fermentation and then a trace of fresh yeast.

He expressed his conclusions as follows: "I am just establishing that in beer-yeast it is not the globules that play the principal part, but the conversion into globules of their soluble part; because I prove that one can suppress the globules that are formed and the total effect on the sugar remains sensibly the same. Thus, certainly, it

[1] *A. de Ch. et de Ph. 3e série*, 52, p. 417.

[2] *The Life of Pasteur*, p. 85.

[3] *Comptes Rendus*, 45, p. 1032.

matters little if one suppresses them by means of filtration with the separation of their soluble part, or if one kills them by a temperature of 100° and leaves them mixed with this soluble part."[1]

In view of the fact that he was supposed to be reasoning on the hypothesis that yeast is organised and living, there was so much that was extraordinary in this that he pauses to reply to inevitable criticism.

"But how, it will be asked, can the fermentation of sugar take place when yeast is used that is heated to 100°, if it is due to the organisation of the soluble part of the globules and these have been paralysed by a temperature of 100°? Fermentation then takes place as it does in a natural sugared liquid, juice of the grape, of sugar-cane, etc., that is to say, *spontaneously*. . . ."

Here is seen the prevalent idea of spontaneous alteration, though Pasteur goes on to state that "in all cases, even those *most liable in appearance* to drive us from belief in the influence of organisation in the phenomena of fermentation, the chemical act that characterises them is always correlative to a formation of globules."

His final conclusions are held up for admiration: "The splitting of sugar into alcohol and carbonic acid is an act correlative of a vital phenomenon, of an organisation of globules, an organisation in which sugar plays a direct part by furnishing a portion of the elements of the substance of these globules." But far from understanding this process, we find that Pasteur owns three years later, in 1860: "Now in what does this chemical act of decomposition, of the alteration of sugar consist? What is its cause? I confess that I am entirely ignorant of it."

In any case, the critical mind inquires at once—How can fermentation be explained as a vital act by the opera-

[1] *Comptes Rendus*, 45, p. 1034. "*Je viens d'établir que dans la levûre de bière, ce ne sont point les globules qui jouent le principal rôle mais la mise en globules de leur partie soluble; car je prouve que l'on peut supprimer les globules formés, et l'effet total sur le sucre est sensiblement le même. Or, assurément, il importe peu qu'on les supprime de fait par une filtration avec séparation de leur partie soluble ou qu'on les tue par une température de 100 degrès en les laissant mêlés à cette partie soluble.*"

tion of a dead organism ; or by the conversion into globules of its soluble part, whatever that may mean; or by spontaneous alteration? No wonder that Béchamp comments[1]: "Pasteur's experiments were so haphazard that he, who acknowledged with Cagniard de Latour the fact of the organisation and life of yeast, *boiled* this living being to study its soluble part!" Indeed, Béchamp's account of Liebig's and Pasteur's closely allied work is well worth perusal from p. 56 to p. 65 of *Les Grands Problèmes Médicaux*.

The chief point to be noted is that as Pasteur made use for these experiments of substances with life in them, such as yeast broth, etc., they could not, in any case, furnish evidence as to the foremost question at stake, namely, whether life could ever arise in a purely chemical medium. That problem was never so much as touched upon by Pasteur in 1857. If we had only his explanation of fermentation, made during that year, we should indeed have a strange idea of the phenomenon. We should believe in the spontaneous generation of alcoholic, lactic and other ferments. We should be puzzled to understand how fermentation could be a vital act and yet be effected by dead organisms. Of the air-borne origin of ferments we should not have an inkling, that is, so far as Pasteur was concerned, for, either, he was ignorant of, or else, he ignored the truth already propounded by others, particularly by Schwann, the German. Pasteur passed over with slight allusion the contacts with air that were involved in his experiments, because his aim was to disprove Liebig's theory that the alteration of yeast broth was due to an oxidation by air and he seems to have had no idea of the important part that air might play, although for a very different reason from the one imagined by Liebig.

Clearly, in 1857, Pasteur was a Sponteparist, without, however, shedding light upon the controversy. The housewife, puzzled by the souring of milk, could only have learned from him that living globules had put in a spontaneous appearance, which explanation had held

[1] *Les Grands Problèmes Médicaux*, p. 60.

good many years earlier to account for the maggots found in bad meat, until it had occurred to the Italian, Francesco Redi, to keep flies from contact.

Here the reader may interpolate that Pasteur's vision, although still obscured, was gradually piercing the fogs of the mystery. But, as it happened, those fogs were by this time dispersed: a "beacon experiment" was shedding light on the difficulty. In 1855 and in 1857 there had been presented to the French Academy of Science Memoirs that were to prove the lode-star of future science, and it seems high time that now, more than half a century afterwards, credit should be given where credit is due in regard to them. And here let us turn to the outcome of work undertaken in a quiet laboratory by one who, perhaps unfortunately for the world, was no adept in the art of advertisement and was too much immersed in his discoveries to be at that time concerned about his proprietary right to them. Let us again open the old French documents and see for ourselves what Professor Antoine Béchamp had to say on the subject of the vexed question of fermentation.

CHAPTER IV

Béchamp's Beacon Experiment

We may recall the fact that it was in the Alsatian capital, Strasbourg, that Professor Béchamp achieved his first scientific triumphs to which we have already alluded. It was there, during the course of his chemical studies, that the idea occurred to him to put the popular belief in the spontaneous alteration of cane-sugar into grape-sugar[1] to the test of a rigid experiment. In those days, organic matter derived from living bodies, whether vegetable or animal, was looked upon as being dead and, according to the views held at that time, because dead liable to spontaneous alteration. This was the belief that Pasteur combated in the way that we have already criticised. Béchamp was before him in attacking the problem by methods obviously more rigid and with results that we think will now appear to be considerably more illuminating.

An experiment upon starch made Béchamp doubt the truth of the popular theory that cane-sugar dissolved in water was spontaneously transformed at an ordinary temperature into invert-sugar, which is a mixture of equal parts of glucose and fructose, the change being technically known as the inversion of sugar. Here was a puzzle that needed investigation and, in attacking this chemical mystery, the Professor had no suspicion of the biological results that were to ensue from Nature's answers.

In May, 1854, he started a series of observations to which he, later on, gave the name of "*Expérience Maitresse*," and finally agreed to call his "Beacon Experiment."

It was on the 16th May, 1854, that the first of the series was commenced in the laboratory of the School of Phar-

[1] See note to p. 34.

macy in Strasbourg. The experiment was concluded on the 3rd February, 1855.

In this experiment perfectly pure cane-sugar was dissolved in distilled water in a glass bottle with an air-tight stopper, but containing a little air. This was left on the laboratory table at ordinary temperature and in diffused light.

At the same time, control experiments were prepared. These consisted of solutions of similar distilled water and cane-sugar, to one of which was added a little zinc chloride, and to the others a little calcium chloride; in each one a small amount of air was left, just as in the bottle containing the first, or test, solution. These bottles were stoppered in the same way as the first, and all were left alongside each other in the laboratory.

In the course of some months, the cane-sugar in the distilled water was partially transformed into grape-sugar, and the polarimeter showed that alteration had taken place in the medium, since there was a change in the angle of rotation. In short, an alteration had taken place, but possibly not spontaneously, for on the 15th of June moulds had put in an appearance and from that date alteration progressed much more rapidly.

The following table I on page 45 is a brief summary of the results of Béchamp's experiments.

[1]TABLE I

Béchamp's Beacon Experiment.

Béchamp prepared solutions of Cane Sugar 16.365 grams in 100 cubic centimetres of various solvents and polarised each of these solutions several times at varying intervals obtaining certain variations in the angle of rotation.

16.365 grm. of Cane Sugar dissolved in 100 c. c. of each of the following :—	Rotation May 16th 1854	Rotation May 17th 1854	Rotation May 20th 1854	Rotation June 15th 1854	Rotation August 20th 1854	Rotation Feb. 3rd 1855	Remarks
1. Distilled Water .	23.88°	23.17°	22.85°	22.39° *	17.28°	7.80°	*Moulds appeared but did not greatly increase.
2. 25% solution of Chloride of Zinc .	22.32°	22.20°	22.10° **	22.14°	22.27°	22.28°	**The Solution began to get cloudy. Later there was found a slight deposit of Oxy-Chloride of Zinc.
3. A solution of Calcium Chloride containing an amount of Calcium Chloride equivalent to the Chloride of Zinc.[2]	22.34°	22.13°	22.17°	22.25°	22.22°	22.29°	No moulds appeared.
4. 25% solution of Calcium Chloride.	22.34°	22.15°	22.10°	22.08°	22.14°	22.28°	No moulds appeared.

[1] *Les Microzymas*, p. 48.

[2] The original is "*Solution de chlorure de calcium équivalente au poids du chlorure de zinc*". From this it is inferred that the concentration of $CaCl_2$ was molecularly equivalent, i.e.

$$25\% \times \frac{\text{molecular weight of } CaCl_2}{\text{molecular weight of } ZnCl_2}; \text{ i.e.}$$

$$25\% \times \frac{111.0}{136.3} = 20\%.$$

Professor Béchamp took particular note of the moulds, and found it significant that none had appeared in the solutions to which he had added zinc chloride and calcium chloride, moreover, that the change in rotation in these had been so slight as to be almost negligible, or, as he puts it: "The plane of polarisation underwent no change other than accidental variations."[1]

Béchamp published this experiment in the Report of the French Academy of Science on the 19th February, 1855.[2] He mentioned the moulds, without attempting to explain their appearance. He reserved their further consideration for future experiments; feeling it important to find the explanation as a probable clue to the cause of what had, up to that time, been regarded as evidence of spontaneous generation. He was also anxious to discover what was the chemical mechanism of the alteration of sugar, and why a change had not been effected in the solutions to which the chlorides had been added.

Meanwhile, another observer, M. Maumené, was also experimenting, and though Béchamp disagreed with his conclusions, he was much struck by the observations that were presented to the Academy of Science on the 7th April, 1856, and published in the *Annales de Chimie et de Physique* in September, 1856.[3]

M. Maumené's experiments were also concerned with polarimetric measurements. The following table II on page 47 gives a brief resumé of his principal results:—

[1] *Les Microzymas, par A. Béchamp*, p. 48.

[2] *Comptes Rendus* 40, p. 436.

[3] *A. de Ch. et de Ph. 3e série*, 48, p. 23.

[1]TABLE II

Experiment by M. Maumené.

Variety of sugar 16.35 gr. in 100 c.c. of solution	Initial rotation in 200 m.m. tube January 4th, 1854	Rotation at the end of 9 months in 200 m.m. tube	Remarks
White candy	+100°	+22°	Slight mould.
Another sample	+100°	+23°	Idem.
Loaf sugar	+98.5°	+31.5°	Mould a little larger.
Another sample	+96.5°	+88°	Slight mould.

Béchamp here saw his own observations borne out. On pages 50 and 51 of *Les Microzymas* he tells us the two questions that had arisen in his mind through his own and M. Maumené's experiments:

"Are moulds endowed with chemical activity?"

"What is the origin of the moulds that appear in the sugared water?"

With a view to finding an answer to these questions, he commenced at Strasbourg on the 25th June, 1856, a fresh series of experiments that were completed at Montpellier on the 5th December, 1857. Thus it was during the course of this work that he left Strasbourg to start his happy successful career at the famous Southern University.

The following table III on page 48 shows his new observations:—

[1] *Les Microzymas*, p. 50.

[1]TABLE III

Béchamp's Beacon Experiment.

15.1 grm. of Cane-sugar dissolved in 100 c.c. of water either with or without the addition of certain chemical substances.	Rotations of the Plane of Polarisation.						Observations
	June 25th 1856	July 13th 1856	Nov. 26th 1856	Mar. 19th 1857	July 13th 1857	Dec. 5th 1857	
Pure water	+ 22.03°	+ 21.89°	+ 16.6°	+ 15.84°	+ 10.3°	+ 1.5°	Nov. 26 a slight flocculent deposit which became gradually a bulky mould.
Very pure arsenious acid, very little	+ 22.04°	+ 21.65°	+ 12.24°	+ 10.8°	+ 7.2°	+ 0.7°	Mould on Nov. 26 which increased and became more abundant than in solution of sugar alone.
Mercuric chloride, very little	+ 22.03°	+ 22.0°	+ 21.9°	+ 22.03°	+ 22.04°	+ 22.1°	Liquid remains transparent.
Pure water, creosoted, one drop .	+ 22.03°	+ 22.0°	+ 22.1°	+22.2°	+ 22.2°	+ 22.2°	Idem.
Sulphate of zinc	+ 22.04°	—	— 3.12°	—	— 7.2°	—	Idem.
Sulphate of aluminium	+ 22.02°	—	— 8.7°	—	—	—	Nov. 26 large green mould.
Nitrate of Potassium	+ 22.05°	+ 21.6°	+ 3.0°	—	—	—	Enormous quantity of moulds developed Nov. 26.
Nitrate of zinc .	+ 22.01°	+ 22.0°	+ 22.1°	—	+ 22.0°	+ 22.2°	Liquid limpid.
Phosphate of sodium........	+ 20.23°	+ 19.16°	— 9.7°	—	—	—	Nov. 22 a bulky mould.
Carbonate of potassium	+ 20.0°	+ 20.0°	+ 20.0°	—	+ 20.3°	—	Liquid remains limpid.
Oxalate of potassium	+ 22.0°	+ 20.34°	+ 10.5°	—	—	— 0.2°	Red moulds.

[1] *Les Microzymas*, p. 52.

The results clearly demonstrated the varying effects of different salts upon the medium, which Béchamp himself has pointed out in the second chapter of his work *Les Microzymas*. As also shown by the earlier experiment, zinc chloride and calcium chloride prevented the alteration of cane-sugar, and a very small quantity of creosote, or of mercuric chloride, had the same preventive influence. This was not the case with arsenious acid when present in very small proportion, or with certain other salts, which did not hamper the appearing of moulds and the alteration of the cane-sugar. Indeed, some of the salts seemed to stimulate the advent of moulds; while, on the contrary, creosote, which has only since the date of these experiments been distinguished from carbolic acid, was particularly effective in the prevention of moulds and of alteration of the sugar.

With his characteristic precision, Professor Béchamp determined to investigate thoroughly the rôle of creosote, and with this aim in view started on the 27th March, 1857, another series of experiments, which he also continued up to the 5th December of the same year.

His own account of his procedure is as follows:[1] He "prepared several sugared solutions according to the technique of the anti-heterogenists, that is to say, the water used was boiled and cooled in such a manner that air could enter only after passing through tubes containing sulphuric acid. This water dissolved the sugar very rapidly, and several jars were completely filled with the carefully filtered solution, so as to leave no air in them. Another part of the solution, having creosote added to it, was poured into jars in contact with a considerable quantity of common air, without any other care than that of cleanliness. One of the jars contained also some arsenious acid. One jar of the creosoted solution and one without cresote were set apart not to be opened throughout the whole course of the experiment."

[1] *Les Microzymas, par A. Béchamp*, p. 53.

The following table IV gives a summary of the observations:—

[1]TABLE IV

Béchamp's Beacon Experiment.

16.365 grm. of Cane-sugar in 100 c.c.	Rotations of the Plane of Polarisation.						Observations
	1857 Mar. 27	April 30	May 30	June 30	July 30	Dec. 5	
Solution not creosoted (No. 1)	+24°	+24°	+24°	+23°	—	+ 19.68°	Whitish floccula carpeted the bottom of the flasks.
Id. (No. 2)	+24°	+24°	+22.8°	+21.6°	—	+15.6°	In flask No. 2 the floccula became more abundant; June 30, without filtering, one drop of creosote was added; this addition did not prevent the further progress of the inversion.
Id. (No. 3)	+24°	—	+24°	—	—	—	
Id. (No. 4)	+24°	—	—	+24°	+24°	—	
Id. (No. 5)	+24°	—	—	—	—	+24°	
Creosoted solutions (No. 1a)	+24°	+24°	+24°	+24°	+24°	+24°	
Id. (No. 2a)	+24°	—	+24°	+24°	+24°	+24°	
Id. (No 3a)	+24°	—	—	+24°	+24°	+24°	
Id. (No. 4a)	+24°	—	—	—	+24°	+24°	
(No. 5a)	+24°	—	—	—	—	+24°	
Creosoted arseniated solution .	+24°	+24°	—	+24°	+24°	+24°	

Béchamp has himself explained the results.

Flasks 1 and 2 lost a little liquid during manipulation and thus were not completely filled. Air in consequence came into contact with the solutions they held and, in these, moulds appeared and alteration in the medium ensued, the dates differing in the two cases and the variation proving more rapid in the flask where the moulds were the more abundant.

On the contrary, the sugared water quite secured from air during the eight months of observation underwent no change, although kept in the warm climate of Montpellier

[1] *Les Microzymas*, p. 54.

during the months of June, July, August and September. This was noteworthy, for there was nothing to prevent the action of the water, had spontaneous alteration been Nature's method, according to the then prevalent opinion. Furthermore, although the creosoted solutions were in contact with air from the start, and these particular flasks were left open, they underwent no variation and showed no trace of moulds, not even the solution to which arsenious acid had been added.

Finally, to return to solution No. 2, moulds appeared before May 30th, with evidence on that date of a diminution of the rotation, which continued to decline, in spite of the fact that on June 30th one drop of creosote was added.

The great worker tells us in his Preface to his work *Le Sang* that these different observations impressed him in the same way as the swing of the cathedral lamp had impressed Galileo in the sixteenth century.

At the period in which he worked, it was believed that fermentation could not take place except in the presence of albuminoid matter. We have already seen that Pasteur operated with yeast broth, a complex albuminoid solution. In the media prepared by Béchamp there were, on the contrary, no albuminoid substances. He had operated with carefully distilled water and pure cane-sugar which, so he tells us, when heated with fresh-slaked lime, did not disengage ammonia. Yet moulds, obviously living organisms and thus necessarily containing albuminoid matter, had appeared in his chemical solutions.

He was awestruck by his discovery, his genius already affording him hints of all it portended. Had he been Pasteur, the country would have rung with the news of it; he would have described the facts by letter to all his acquaintances. Instead, being Béchamp, without a thought of self, immersed in the secrets Nature disclosed, his only anxiety was to start new experiments, consider fresh revelations.

The results of the observations he recorded in a Memoir, which he sent up immediately, in December, 1857, to the

Academy of Science, which published an extract of it among its Reports of the 4th January, 1858.[1] The full publication of this all-important document was actually, for some unknown reason, deferred for eight months, when it appeared in September, 1858, in the *Annales de Chimie et de Physique*.[2]

The title of the Memoir was: "On the influence that Water, either Pure or Charged with Various Salts, Exercises in the Cold upon Cane-Sugar."

Béchamp thus comments upon this:[3] "By its title the Memoir was a work of pure chemistry, which had at first no other object than to determine whether or no, pure cold water could invert cane-sugar, and if, further, the salts had any influence on the inversion; but soon the question, as I had foreseen, became complicated; it became at once physiological and dependent upon the phenomena of fermentation and the question of spontaneous generation —thus, from the study of a simple chemical fact, I was led to investigate in my turn the causes of fermentation, the nature and origin of ferments."

The main sweeping result of all the experiments went to prove that "Cold Water modifies Cane-Sugar only in Proportion to the development of Moulds, these Elementary Vegetations then acting as Ferments."[4]

Here at one stroke was felled the theory of alteration through the action of water, the change known as fermentation being declared to be due to the growth of living organisms.

Furthermore, it was proved that "Moulds do not Develop when there is no Contact with Air and that no Change then takes Place in the Rotary Power"; also that "The Solutions that had Come in Contact with Air Varied in Proportion to the Development of Moulds." The necessity of the presence of these living organ-

[1] *Comptes Rendus* 46, p. 44.

[2] *A. de Ch. et de Ph. 3e série*, 54, p. 28.

[3] *Les Microzymas, par A. Béchamp*, p. 55.

[4] *Comptes Rendus*, 46, p. 44.

isms for the processes of fermentation was thus shown clearly.

Béchamp further explained the action of moulds: "They act after the manner of ferments."

"Whence comes the ferment?"

"In these solutions there existed no albuminoid substance; they were made with pure cane-sugar, which, heated with fresh-slaked lime, does not give off ammonia. It thus appears evident that *air-borne germs* found the sugared solution a favourable medium for their development, and it must be admitted that *the ferment is here produced by the generation of fungi*."

Here, in direct contradiction to Pasteur's account of the spontaneous origin of beer-yeast and other organisms, Béchamp gave proof positive of Schwann's teaching of air-borne germs, and further specified yeast to be of the order of fungi. Remarkable though such a clear pronouncement was at a date when scientific ideas were in chaotic confusion, the great teacher went much further afield in his observations.

Moreover he stated:—"The matter that develops in the sugared water sometimes presents itself under the form of little isolated bodies, sometimes under the form of voluminous colourless membranes, which come out in one mass from the flasks. These membranes, heated with caustic potash, give off ammonia in abundance."

Here he noted the diversity of the organisms of these moulds, an observation that was to result in a deep insight into cellular life, and his foundation of a first proper understanding of cytology.

He had a further definite explanation to make on the action of moulds, namely:—"The Transformation that Cane-Sugar Undergoes in the Presence of Moulds may be Compared with that Produced upon Starch by Diastase."

This particular conclusion, he tells us,[1] had an enormous bearing on the subject, and was such a novel idea at that epoch that Pasteur, even later, ignored and denied it.

[1] *Les Microzymas, par A. Béchamp*, p. 57.

He further explained that "Cold water does not act upon cane-sugar except when moulds are able to develop in it; in other words, the transformation is due to a true fermentation and to the development of an acid that is consecutive to the appearance of the ferment."

It was by the acids engendered by the moulds that he explained the process of fermentation.

He had many more conclusions to put forward as to the effect of various salts upon the solutions. Had Lord Lister only followed Béchamp's teaching in regard to creosote, instead of Pasteur's, the former might have been spared his subsequent honest recantation of his invention, the carbolic spray, which proved fatal to many patients.

Béchamp taught that "Creosote in Preventing the Development of Moulds also Checks the Transformation of Cane-Sugar."

He also taught that "Creosote, with or without prolonged contact with air, prevents at one and the same time the formation of moulds and the transformation of cane-sugar. But from observation it appears that, when the moulds are once formed, creosote does not prevent their action."

He drew many more conclusions from the effects of different salts and thus generalised:—"The influence of saline solutions is variable, not only according to the sort or kind of salt, but moreover according to the degree of saturation and of neutrality of these salts. The salts that prevent the transformation of cane-sugar into glucose (grape-sugar) are generally the salts reputed to be antiseptic. In all cases, a certain minimum temperature is necessary for the transformation to take place."

Thus, we see that at that early date, 1857, when fermentation was such a complete mystery that Pasteur, operating with albuminoid matters, including dead yeast, looked upon this yeast and other organisms as products of spontaneous generation, Béchamp sent out an all-comprehending searchlight which illumined the darkness of the subject for all time.

To recapitulate, in a short summary, he taught that cane-sugar was a proximate principle unalterable by solution in water. He taught that the air had in itself no effect upon it, but that owing to its importation of living organisms, the apparent effect of air was all-important. He showed that these organisms, insoluble themselves, brought about the process of fermentation by means of the acids they generated, which acids were regarded as the soluble ferments. He taught that the way to prevent the invasion of organisms in the sugared solution was by first slightly creosoting the medium; but if the organisms had appeared before creosote was added, he showed that its subsequent addition would have no power to arrest their development and the consequent inversion of the sugar.

For further revelations, we cannot do better than quote two or three paragraphs from Béchamp's own summary of his discovery in the Preface to his last work *Le Sang—The Blood*.[1]

There he writes:—"It resulted that the soluble ferment was allied to the insoluble by the reaction of product to producer; the soluble ferment being unable to exist without the organised ferment, which is necessarily insoluble.

"Further, as the soluble ferment and the albuminoid matter, being nitrogenous, could only be formed by obtaining the nitrogen from the limited volume of air left in the flasks, it was at the same time demonstrated that the free nitrogen of the air could help directly in the synthesis of the nitrogenous substance of plants; which up to that time had been a disputed question.[2]

"Thus it became evident that since the material forming the structure of moulds and yeasts was elaborated within the organism, it must also be true that the soluble ferments and products of fermentation are also secreted there, as was the case with the soluble ferment that inverted the cane-sugar. Hence I became assured that that which is

[1] p. 16.

[2] It is now considered that atmospheric nitrogen can only be utilized by a few special plants (Natural order—Luguminosæ) and then under special conditions.

called fermentation is, in reality, the phenomenon of nutrition, assimilation, disassimilation and excretion of the products disassimilated."[1]

Thus we see how clear and complete was Béchamp's explanation of fermentation so long ago as the year 1857. He showed it to be due to the life processes of living organisms so minute as to require a microscope to render them visible and in the case of his sugared solutions he proved them to be air-borne. Not only was he incontestably the first to solve the problem, but his initial discovery was to lead him a great deal further, unfortunately far beyond the understanding of those who, devoid of his insight of genius, became merely obsessed by the idea of atmospheric organisms. But before we proceed to delve deeper in Béchamp's teaching, let us pause and return to Pasteur and see how his work was affected by the great beacon wherewith his rival had illumined science.

[1] In modern phraseology these processes are known as nutrition, constructive metabolism, destructive metabolism and the excretion of the waste products of the last named process.

Who Proved Fermentation in a Chemical Medium to be due to Air-borne Living Organisms :—

BÉCHAMP or PASTEUR?

BÉCHAMP	PASTEUR
1855[2] and 1857[3]	1857[4]
	LACTIC FERMENTATION
Experiments upon perfectly pure cane-sugar in distilled water, with or without the addition of different salts, air in some cases excluded, in others admitted.	Experiment with ferment obtained from a medium of sugar, chalk, caseine or fibrin and gluten and sown in yeast broth (a complex solution of albuminoid and mineral matters) in which sugar had been dissolved with the addition of chalk.
(2) *Comptes Rendus de l'Académie des Sciences* 40, p. 436. (3) *C. R. 46, p. 44.* See also *Annales de Chimie et de Physique, 3e série*, 54, p. 28.	(4) *Comptes Rendus de l'Académie des Sciences* 45, p. 913.

BÉCHAMP

CONCLUSIONS:

That the inversion of cane-sugar is due to moulds, which are living organisms, imported by the air, and whose influence upon cane-sugar may be compared with that exercised upon starch by diastase. That creosote prevents the invasion of moulds, though it does not check their development when once established.

. . . .

PASTEUR

CONCLUSIONS:

A lactic ferment *takes birth spontaneously*, as easily as beer-yeast, in the body of the albuminoid liquid furnished by the soluble part of the yeast. The lactic ferment is a living being, though this conclusion is among an order of things that *cannot be irrefutably demonstrated.*

ALCOHOLIC FERMENTATION [1]

Experiment with two equal quantities of fresh yeast washed in water. One was left to ferment with pure sugared water, and after extracting from the other all its soluble part by boiling it with plenty of water and filtering it to get rid of the globules, as much sugar was added to the limpid liquor as was added in the first fermentation, and then a trace of fresh yeast.

CONCLUSIONS:

That in beer-yeast it is not the globules that play the principal part, but the conversion into globules of their soluble part, since the globules may be killed by a temperature of 100° when fermentation takes place spontaneously. The splitting of sugar into alcohol and into carbonic acid is an act correlative of a vital phenomenon.

. . . .

(1) *Comptes Rendus, 45, p. 1032.* See also *Annales de Chimie et de Physique, 3e série*, 52, p. 404.

BÉCHAMP

COROLLARY

That here was the first clear explanation and proof of the mystery of fermentation and the basic foundation of the knowledge of antiseptics.

PASTEUR

COROLLARY

The albuminoid substances, used in these experiments, in themselves nullified the attemps to probe the mystery of changes in a purely chemical medium. The origin of the ferments was said to be spontaneous, and while fermentation was declared to be a *vital* act, *dead* yeast was made principal use of, and the conclusions in general were pronounced to be beyond the power of proof.

CHAPTER V

Claims and Contradictions

Professor Béchamp's great series of observations, which indeed seem to merit the name of the "Beacon Experiment," clearly demonstrated the possibility of the appearance of ferments in a medium devoid of albuminoid matter. As this fact had been disbelieved till this date, it is evident that Béchamp was the first to establish it. We may search through the old scientific records and fail to find any such demonstration by anyone. We can read for ourselves that Pasteur's procedure in 1857 was entirely different. Influenced by the prevalent belief, what he did, as we have already seen, was to take the ferment developed in an ordinary fermentation and sow it in yeast broth, a complex solution of albuminoid and mineral matters. Thus he obtained what he called his lactic fermentation. Neither does he seem to have been entirely successful in his deductions from his observations. He announced that the lactic globules "take birth spontaneously in the body of the albuminoid liquid furnished by the soluble part of the yeast," and also that "they take birth spontaneously with as much facility as beer-yeast." There can be no question of the contrast between these sponteparist views and the clear, simple, explanation of Béchamp! No conscientious reader can compare the two workers' original documents without being struck by their disparity.

Where Pasteur's work was more allied to Béchamp's was in an experiment recorded among the Reports of the French Academy of Science in February, 1859, more than a year after the publication of Béchamp's Beacon Experiment. So, certainly, from the point of date alone, it in no

way repudiates Béchamp's claim to priority in clearly explaining fermentation; indeed, it seems to have been inspired by the Professor's observations, for we find that Pasteur here omitted to use yeast broth as his medium, and ascribed the origin of lactic yeast to the atmospheric air.

According to his own details,[1] he mixed with pure sugared water a small quantity of salt of ammonia, phosphates and precipitated carbonate of lime, and actually expressed surprise that animal and vegetable matter should have appeared in such an environment. There could hardly be a greater contrast to Béchamp's rigorous deductions, while an extraordinary ambiguity follows in the conclusions. We read:—"As to the origin of the lactic yeast in these experiments, it is solely due to the atmospheric air: we fall back here upon facts of spontaneous generation." After asserting that by suppressing all contact with ordinary air, or by boiling the solution, the formation of organisms and fermentation are quite prevented, he winds up:—"On this point the question of spontaneous generation has made progress." If he here meant that the question had progressed towards the denial of the belief, why was it that he did not say so?

In a subsequent Memoir published in the *Annales de Chimie et de Physique*[2] in April, 1860, he constantly refers to the spontaneous production of yeasts and fermentations. Anyone really aware of the atmospheric origin of micro-organisms of the nature of yeast would, undoubtedly, have steered clear of phraseology that, at that particular epoch, conveyed such a diametrically opposite signification.

The many experiments detailed in this latter Memoir were only commenced on the 10th December, 1858, whereas Béchamp first presented his Beacon Experiment to the Academy of Science in December, 1857, and its full publication appeared in September, 1858, three months before Pasteur started his fresh observations. He was, undoubtedly, inspired by Béchamp in this new work for

[1] *Comptes Rendus* 48, p. 337.

[2] *3e série*, 57-58, pp. 323 to 426 inclusive, esp. from pp. 283 to 392.

which he made claim that it illumined "with a new day the phenomena of fermentation."

Béchamp's criticism of it may be found in the Preface to his book, *Le Sang*. There he explains that the formation of lactic acid, following upon the original alcoholic fermentation, was due to an invasion by atmospheric germs, in this case lactic yeast, their subsequent increase resulting in the starvation of the beer-yeast, which had been included at the start of the experiment. He maintains that Pasteur's deductions prove his lack of real comprehension of "the chemico-physiological phenomena of transformation, called fermentation, which are processes of nutrition, that is to say, of digestion, followed by absorption, assimilation, excretion, etc.," also his want of understanding of the living organism and how it would "at last reproduce itself if all conditions dependent upon nutrition are fulfilled."[1]

Over and above Béchamp's scientific criticism of this Memoir, any critic must be struck by the inexactitude of the detailed descriptions. For example, if we turn to the third section, we find that for these observations, Pasteur's medium included the ashes of yeast, and that he makes mention of the addition of fresh yeast. Yet as a heading to one such experiment, he gives the following misleading description: "Production of yeast in a medium formed of sugar, of a salt of ammonia and of phosphates."[2] All reference to the original inclusion of yeast, admitted on p. 383, is omitted in this heading and in his final summary:—"All these results of a most rigorous exactitude, though the majority were obtained by acting upon very small quantities, establish the production of alcoholic and lactic yeast and of special fermentations corresponding to them, in a medium *formed only* of sugar, a salt of ammonia and of mineral elements."[3] The actual medium detailed only a couple of pages back, consisted of:—

[1] *Le Sang, par A. Béchamp*, Preface, p. 41.

[2] *Annales de Chimie et de Physique, 3e série*, 57-58, p. 381.

[3] *ibid. 3e série*, 57-58, p. 392.

"10 grammes of sugar.
100 cubic centimetres of water.
0.100 grm. of ammonium tartrate.
The ash from 1 gramme of beer-yeast.
Traces of *fresh yeast*, the size of a pin's head."[1]

Altogether, it is clear that even by 1860, Pasteur had no such clear teaching to put forward as that contained in Béchamp's epoch-making observations. And here we have an illuminating view of the characters of the two men. Béchamp could not but be aware that his knowledge exceeded that of Pasteur; yet, all the same, in his lectures before students, we find nothing but courteous allusions to his rivals. We need only refer to the Professor's *Lessons on Vinous Fermentation*, a work published in 1863, before his actual demonstration in explanation of the phenomenon.

In this book, we learn Béchamp's views, which he was so careful always to carry into practice, on the subject of giving honour where honour is due in scientific revelations. "One can only have," he says,[2] "inspired ideas or communicated ideas, and it is by working upon one and the other that new ones are conceived. That is why a seeker after truth should give the ideas of those who preceded him in his work, because those, great or small, had to make their effort, and herein lies their merit, to bring their share of truth to the world. I cannot conceive of a superior title than this of proprietary right, because it is this that constitutes our personality and often genius, if it be true that this sublime prerogative, this rare privilege, is nothing but a long patience, or, to be more correct, a work of perseverance, fecundated by the spark God has set in us. This right must be respected all the more, in that it is of the nature of the only riches, the only property, that we can lavish without impoverishing ourselves; what say I,

[1] *Annales de Chimie et de Physique*, p. 390.
"10 grammes de sucre
100 centimètres cubes d'eau
Ogr. 100 de tartrate droit d'ammoniaque
Cendres de 1 gramme de levûre
Traces de levûre fraîche (de la grosseur d'une tête d'épingle)."

[2] *Leçons sur la Fermentation Vineuse et sur la Fabrication du Vin, par A. Béchamp*, pp. 6, 7.

it is in thus spending it that we enrich ourselves more and more."

Unfortunately, we find a great contrast in Pasteur, who, it cannot be gainsaid, from the start, according to the old records, repeatedly arrogated to himself the discoveries of Béchamp, beginning with those of 1857.

The Beacon Experiment had flashed illumination into the darkness of sponteparist views just at a time when the controversy on spontaneous generation was destined to flame out anew. At the end of December, 1858, M. Pouchet, Director of the Natural History Museum of Rouen, sent up to the Academy of Science a "Note on Vegetable and Animal Proto-Organisms Spontaneously Generated in Artificial Air and in Oxygen-Gas." The subject again gripped public interest. Professor Béchamp, seizing every spare moment for continued research, was too much occupied working to take much part in talking. Pasteur, on the contrary, kept everyone well acquainted with the experiments he purposed to undertake. There were said to be living organisms, germs, in the atmosphere; so he decided microscopically to investigate air. The method of doing so, by filtering it into glass flasks, had already been inaugurated by two Germans, Schroeder and Dusch. Experimenting in the same way, Pasteur made comparisons between the different contents of phials, which, according to him, varied with the admission of atmospheric dust and remained unaltered in examples where this was excluded. But he was not content with laboratory and cellar experiments and planned to make observations that would be more striking and picturesque. Keeping everyone well notified of his proceedings, in September, 1860, he started on a tour, armed with 73 phials, which he opened and then summarily sealed at different places and at varying altitudes. The last 20 he reserved for the *Mer de Glace* above Chamonix, with the result that in only one of the twenty were the contents found to be altered. From this time, the autumn of 1860, Pasteur, the former Sponteparist, veered round to a com-

pletely opposite standpoint, and ascribed almost all phenomena to the influence of atmospheric germs.

His immediate opponent, meanwhile, experimented on air on mountains, on plains, on the sea, and, as everybody knows, Pasteur never succeeded in convincing M. Pouchet.

Of these Pasteurian experiments, Béchamp writes:[1]—"From his microscopic analysis, he comes to conclusions, like Pouchet, without precision (*sans rien préciser*); there are organised corpuscles in the collected dust, only he cannot say "this is an egg, that is a spore," but he affirms that there are a sufficient number to explain all the cases of the generation of infusoria. Pasteur thus took up the position of explaining by germs of the air all that he had explained before by spontaneous generation."

He was naturally entitled to hold any opinions that he chose, whether they were superficial or otherwise, and also to change his opinions, but we think all will agree that what he was not entitled to do was to claim for himself discoveries initiated by another worker. Yet, in a discussion on spontaneous generation, which took place at the Sorbonne, during a Meeting, on the 22nd Nov., 1861, of the *Sociétés Savantes*, Pasteur, actually in the presence of Professor Béchamp, took to himself the credit of the proof of the appearance of living organisms in a medium devoid of albuminoid matter. The Professor, with that distaste for self-advertisement, which so often accompanies the highest intellectuality, listened in amazed silence, until his own turn came, when, instead of putting forward the legitimate seniority of his work, he merely gave an account of the experiments described in his great Memoir and the conclusions that had resulted from them. On returning to his seat, which happened to be next to Pasteur's, he asked the latter to be so kind as to admit his knowledge of the work that had just been under description. The Report of the Meeting tells us of Pasteur's method of compliance.[2]

[1] *Les Grands Problèmes Médicaux, par A. Béchamp*, p. 13.

[2] *Revues des Sociétés Savantes I*, p. 81 (1862).

"M. Béchamp quoted some experiments" (those of the Memoir of 1857) "wherein the transformation of cane-sugar into grape-sugar effected under the influence of the air, is always accompanied by moulds. These experiments agree with the results obtained by M. Pasteur, who hastened to acknowledge that the fact put forward by M. Béchamp is one of *the most rigid exactness*."

We cannot help thinking that Pasteur might also have added an admission that his associate had been in the field before him. A further point to be noticed is Pasteur's later contradiction of his own words, for Béchamp's work, here described by him as rigidly exact, was later to be accused by him as guilty of "an enormity."

We turn to the *Études sur la Bière*:[1] "I must repudiate a claim of priority raised by M. Béchamp. It is known that I was the first to demonstrate that living ferments can be entirely constituted from their germs deposited in pure water into which sugar, ammonia and phosphates have been introduced and protected from light and green matter. M. Béchamp, relying on the old fact that moulds arise in sugared water, and according to him, invert the sugar, pretends to have proved that organized living ferments can arise in media deprived of albuminoid matters. To be logical, M. Béchamp should say that he has proved that moulds arise in pure sugared water without nitrogen, without phosphates or other mineral elements, for *that is an enormity* that can be deduced from his work, in which there is not even the expression of the least astonishment that moulds have been able to grow in pure water with pure sugar without other mineral or organic principles."

How was it then that the present traducer of Béchamp's work should, as we have already shown, have earlier described that self-same work as possessing "rigid exactness?" Can it be that it is only when it is likely to eclipse Pasteur's that it turns into "an enormity"? And how did Pasteur come to omit all reference to the admittance of air,

[1] p. 310 (note).

without which the formation of moulds would have been impossible?

At a time when Pasteur was using yeast broth and other albuminoid matters for his experiments, Béchamp, on the contrary, gave a clear demonstration that in media devoid of albuminoid matters moulds would appear, which, when heated with caustic potash, set free ammonia. By the same set of experiments, the Professor proved that moulds, living organisms that play the part of ferments, are deposited from the air and appear in pure water to which nothing but sugar, or sugar and certain salts, have been added. Therefore by this criticism, "to be logical, M. Béchamp should say that he has proved that moulds arise in pure sugared water, without nitrogen, without phosphates or other mineral elements, for that is an enormity that can be deduced from his work," M. Pasteur seems himself to have committed the enormity by thus apparently misunderstanding the facts proved by Béchamp! The latter had noted that in the glass flasks filled completely with the solution of sugar and distilled water and into which *no air* whatever was allowed to enter, moulds did *not* appear and the sugar was not inverted; but in the flasks in which *air* had remained, or into which it had been allowed to penetrate, moulds had formed, despite the absence of the albuminoid matters included in Pasteur's experiments: moreover Béchamp had found these moulds to be more abundant when particular salts, such as nitrates, phosphates, etc., had been added.

The Professor, in his great work *Les Microzymas*,[1] cannot resist a sarcastic allusion to Pasteur's extraordinary criticism:—"A chemist, *au courant* with science, ought not to be surprised that moulds are developed in sweetened water contained, in contact with air, in glass flasks. It is the astonishment of M. Pasteur that is astonishing!"

When wordy warfare ensued, Pasteur was no match for Béchamp and the former quickly saw that his own interests would be best served by passing over the latter's

[1] p. 87.

work so far as possible in silence. This human weakness of jealousy was no doubt one of the contributory causes of the setting aside of important discoveries, which, afterwards ascribed to Büchner in 1897[1], were actually made by Béchamp before 1864, in which year he first publicly employed the name *zymase* for the soluble ferment of yeasts and moulds. And it is now to these researches of his that we shall do well to turn our attention.

[1] See pp. 19, 74, 75, 162.

CHAPTER VI

The Soluble Ferment

Before we can form any idea of the magnitude of Béchamp's discoveries, we must thoroughly realise the obscurity of the scientific views of the period. Not only were physical and chemical influences believed to be operative in the spontaneous generation of plant and animal life, but Dumas' physiological theory of fermentation had been set aside for the belief that this transformation anteceded the appearance of micro-organisms.

We have already seen that light was thrown upon this darkness by Béchamp's Beacon Experiment; we have now to study the teaching he deduced from his observations.

At the date of the publication of his Memoir, scientists were so little prepared to admit that moulds could appear apart from the co-operation of some albuminoid matter that it was at first insisted that Béchamp must have employed impure sugar. On the contrary, he had made use of pure sugar candy, which did not produce ammonia when heated with soda lime. Yet his critics would not be satisfied, even by the fact that the quantity of ammonia set free by the moulds far surpassed any that could have been furnished by an impurity. Further evidence was given by the experiments that showed the development of micro-organisms in mineral media, and these could not be accused of connection with anything albuminoid.

Béchamp was not, of course, the first to view and notice the moulds, the micro-organisms. That had been done before him. What he did was conclusively to demonstrate their atmospheric origin, and, above all, to explain their function. Anyone interested in this important subject cannot do better than study the second *Conference*, or

chapter, of his great work *Les Microzymas*, where the matter is explained fully. Here we can only briefly summarize some of its teaching.

The outstanding evidence that faced the Professor in his observations was the fact that the moulds, which appeared in sweetened water exposed to air, set free ammonia when heated with caustic potash. This was evidence that a nitrogenized organic substance, probably albuminoid, had been produced and had served to constitute one of the materials necessary for the development of an organised being. Whence had it arisen? The Professor finds his answer by a study of Nature. He describes how the seed of a flowering plant will germinate and the plant that appears will grow and develop, always weighing more than the seed sown originally. Whence were the chemical compounds derived that were wanting in the seed? The answer, he says, is elementary, and he goes on to explain how the organs of the young plant are the chemical apparatus in which the surrounding media (i.e. the water in the soil, in which it strikes its roots, supplying nitrogenous salts, and the atmosphere providing its leaves with carbonic acid and oxygen) are enabled to react and produce according to chemical laws the compounds whereby the plant is nourished and wherewith it builds up its cells and hence all its organs. In the same way behaves the spore of the mucorina, which the air carried to the sweetened solution. It develops, and, in the body of the microscopic plant, the air, with its nutrient contents, the water and the dissolved materials in the sweetened solution react and the necessary organic matter is elaborated and compounds are produced which were non-existent in the original medium. He goes on to explain that it is because the mucorina is a plant, with the faculty of producing organic matter, that it is able to develop in a medium that contains nothing organized. For this production of organic matter, the help of certain minerals is indispensable. Béchamp here reverts to Lavoisier's explanation of the way in which water attacks glass and

dissolves a portion of it, and himself shows how the moulds are thus supplied with the earthy and alkaline materials they need. The amount thus furnished is very small, so that the harvest of moulds is correspondingly limited. If, however, certain salts, such as aluminium sulphate, potassium nitrate or sodium phosphate were added to the sweetened water large moulds resulted, and the inversion of the sugar was proportionately rapid.

"The meaning of this," says Béchamp, "is that each of these salts introduced a specially favourable condition and perhaps helped in attacking the glass, which thus yielded a greater quantity of its own substance."[1]

But, even still, the mystery of fermentation was not quite clear without an explanation of the actual way in which the change in the sugar was brought about, that is to say, cane-sugar transformed into grape-sugar.

Here again, as we have already seen, Béchamp solved the difficulty by a comparison and likened the influence of moulds to the effects exercised upon starch by diastase, which, in solution, possesses the property of causing starch to break up at a high temperature, transforming it first into dextrin and then into sugar.

Béchamp proved his comparison to be correct by rigorous experiments. By crushing the moulds which appeared in his solutions, he found that the cells that composed them secreted a soluble ferment and that the latter was the direct agent in transforming the sugar, and he made a very clear demonstration of this also in regard to beer-yeast. For instance, just in the same way, the stomach does not work directly upon food, but only indirectly through a secretion called gastric juice, which contains pepsin, a substance more or less analogous to diastase, and which is the direct agent of the chemical changes that take place in the digestive organ. Thus, it is by a soluble product that beer-yeast and certain other moulds bring about the chemical change that alters the type of sugar. Just as the stomach could not transform food

[1] *Les Microzymas*, p. 84.

without the juice it secretes, so yeast could not change sugar without a soluble ferment secreted by its cells.

On p. 70 of *Les Microzymas*, Professor Béchamp commences an account of some of the experiments he undertook in this connection. Here may be found the description of an experiment with thoroughly washed and dried beer-yeast, which was mixed with a little more than its weight of cane-sugar and the mixture carefully creosoted, the whole becoming soft and by degrees completely fluid. Béchamp provides a full explanation of the action. He shows that the yeast cell is like a closed vesicle, or a container enclosing a content, and that it is limited in space by a membranous envelope. In the dried state, in which he made use of it for his experiment, it yet contained more than 70 per cent of water, no more perceptible to touch than the amount, on an average 80 per cent of the body-weight, contained in the human body. He explains how the living yeast, in its natural state, on contact with water, allows nothing of its content to escape except excretory products, but, in contact with the sugar, it is, as it were, irritated and the enveloping membrane permits the escape of water with certain other materials held in solution, and it is this fluid that liquefies the mixture of yeast and sugar. The escape of the fluid Béchamp shows to be due to the physical process *osmosis*, by which a solution passes through a permeable membrane. Thus having obtained his liquid product he diluted it with water and left it to filter.

Meanwhile, Béchamp performed another experiment; namely, he dissolved a small piece of cane-sugar in water and found that no change was produced when this was heated with alkaline copper tartrate. He then took another small piece of sugar and heated it to boiling point with very dilute hydrochloric acid; he neutralised the acid with caustic potash and made the solution alkaline, he then added his copper reagent and heated it, whereupon reduction took place, a precipitate being produced which was at first yellow and then red. By means of the acid the sugar had been inverted, that is to say, trans-

formed into a mixture of glucose and levulose (a constituent of fruit sugar), which reduced the cupric copper of the blue reagent to cuprous copper which was precipitated as the red oxide.

Béchamp then returned to the liquid that had been filtering, and found that when he barely heated it with the alkaline copper tartrate reagent the change in the sugar was effected. This proved to him that something besides water had escaped from the yeast, something that, even in the cold, had the power of rapidly inverting the sugar.

Professor Béchamp here points out[1] two facts that must be clearly demonstrated. First, that, without the escaping element, yeast in itself is inoperative, for when steeped in water, with the alkaline copper tartrate reagent added, reduction is not effected. Secondly, that heat destroys the activity of the escaping element, for yeast brought to the boil with a little water to which sugar is added, does not, even after time has been allowed for it to take effect, produce the inversion, the alkaline copper tartrate reagent is not reduced. In short, he discovered that heat destroys the activity of the ferment secreted by yeast and moulds of all sorts, just as heat destroys the activity of sprouted barley, of diastase and of other soluble ferments, that is, ferments capable of being dissolved in a fluid.

Béchamp further discovered sodium acetate to be another agent especially efficient in promoting the passage of the soluble contents through the cell walls. To dried yeast, he added some crystals of that salt, experimenting on a sufficiently large quantity. The mixture became liquid and was thrown upon a filter. One part sodium acetate to ten or more of yeast, he found sufficient to effect the liquefaction. He then took the filtered liquid and added alcohol to it and a white precipitate appeared. He collected this in a filter and washed it with alcohol to free it from the sodium acetate. The alcohol being drained off, the precipitate was dried between folds of filter papers and then it was taken up with water. There

[1] *Les Microzymas*, pp. 71, 72.

resulted a solution and an insoluble residue. This last was coagulated albumen, which came from the yeast in solution, but was rendered insoluble by the coagulating action of the alcohol.

"As to that portion of the precipitate which has been dissolved, alcohol can precipitate it again," says Béchamp.[1] "This new precipitate is to beer-yeast what diastase is to sprouted barley or synaptase to almonds; it is the principle that in the yeast effects the inversion of the cane-sugar. If some of it is dissolved in water, cane-sugar added and the solution kept for several minutes in the water bath at 40°, the alkaline copper tartrate proves that the sugar has been inverted. The action is also very rapid at the ordinary temperature, but slower in proportion to a lesser amount of the active product; which explains the slowness of the reactions obtained with certain moulds that I could only utilise in small quantity. All this proves that the cause of the inversion of the sugar is pre-formed in the moulds and in the yeast, and as the active matter, when isolated, acts in the absence of acid, this shows that I was right in allying it to diastase."

It was after Professor Béchamp had established these facts that he gave a name to this active matter. He called it *zymase*, from the Greek, ζύμη, ferment. The word, applied by him at first to the active matter of yeast and of moulds, has become a generic term. Later on, he specially designated the zymases of yeast and of moulds by the name of *zythozymase*.

Béchamp's first public employment of the name "zymase" for soluble ferments was in a *Memoir on Fermentation by Organised Ferments*, which he read before the Academy of Science on the 4th of April, 1864.[2]

The following year, he resumed the subject[3] and showed that there were zymases in microzoaires and microphytes, which he isolated, as Payen and Persoz isolated the diastase

[1] *Les Microzymas*, p. 72.
[2] *Comptes Rendus* 58, p. 601.
[3] *C. R.* 59, p. 496.

from sprouted barley. These zymases, he found, possessed generally the property of rapidly transforming cane-sugar into glucose, or grape-sugar. He discovered the *anthrozyma* in flowers, the *morozyma* in the white mulberry and the *nephrozyma* in the kidney of animals. Finally, the following year, 1866, he gave the name, *microzyma*,[1] to his crowning discovery, which to him was the basic explanation of the whole question and which had not yet been made apparent to him when he immortalised his early experiments in his Memoir of 1857; but this we must leave for future consideration. We have here given these dates to show how long ago Professor Béchamp made a complete discovery of the nitrogenous substance formed in the yeast cell to which he gave the name of *zymase*.

Apart from the justice of giving credit where credit is due, for the mere sake of historical accuracy it is desirable that his own discovery should be publicly accredited to him. Instead, in the *Encyclopædia Britannica*[2] we find, in the article on "Fermentation" by Julian Levett Baker, F.I.C., that it is stated that "In 1897 Büchner submitted yeast to great pressure and isolated a nitrogenous substance, enzymic in character, which he termed zymase." Again, we take up *A Manual of Bacteriology*,[3] by R. Tanner Hewlett, M.D., F.R.C.P., D.P.H. (Lond.), F.R.M.S., and we read:—"Until 1897 no enzyme had been obtained which would carry out this change (alcoholic fermentation); it only occurred when the living yeast-cells were present, but in that year Büchner, by grinding up the living yeast-cells, obtained a juice which decomposed dextrose with the formation of alcohol and carbonic acid. This 'zymase' Büchner claimed to be the alcoholic enzyme of yeast." Yet once more, Professor and Mrs. Frankland, in their book, *Pasteur*,[4] while apologising for certain of the latter's erroneous views, write as follows:—"In the present year

[1] *Comptes Rendus de l'Académie des Sciences*, 63, p. 451.

[2] Eleventh Edition.

[3] Sixth Edition, p. 36.

[4] See Chapter IX.

(1897) the discovery has been made by E. Büchner that a soluble principle giving rise to the alcoholic fermentation of sugar may be extracted from yeast cells, and for which the name *zymase* is proposed. This important discovery should throw a new light on the theory of fermentation."

But "this important discovery," as we have here seen, was made nearly half a century before by a Frenchman!

It is true that Pasteur accused Béchamp of having taken his ideas from Mitscherlich. Not only was Béchamp able to disprove this, but he also showed that it was Pasteur who had followed the German's views, and that, moreover, on a point on which the latter appeared to have been mistaken.[1]

Thus it is clear that Béchamp was the first to give tangible proof not only of the air-borne origin of yeasts and moulds, but also of the means by which they are physiologically and chemically active. When he started work, there was no teaching available for him to plagiarise, had plagiarism been possible to such a deeply versed and honest student of scientific history, who, step by step, traced any observations that had preceded his own. Unfortunately, it was he who was preyed upon by plagiarists, and, sad to relate, foremost among these seems to have been the very one who tried to detract from his work and who bears the world-famous name of Pasteur! Let us pause here to watch the latter's progress and the way in which he gained credit for Béchamp's great discovery of the invading hordes from the atmosphere, micro-organisms with their fermentative powers.

[1] *Les Microzymas*, pp. 76, 77.

CHAPTER VII

Rival Theories and Workers

Undoubtedly, one of the chief factors of Pasteur's success was the quickness with which he pushed into the forefront of any scientific question, thus focusing public attention upon himself. Béchamp's illuminating explanations of ancient problems were conveniently to hand just at a moment when M. Pouchet brought the controversy on spontaneous generation again into the limelight of general interest. Pasteur, seizing the opportunity, entered the lists, and, as Béchamp comments, M. Pouchet's observations being as wanting in precision as Pasteur's, it was not hard for the latter to emerge as victor, genuinely impressing the world of scientists.

Thus he who had taught the spontaneous origin of yeast and of micro-organisms of all sorts, now discoursed with almost childish enthusiasm upon the germs of the air, and began to make life synonymous with atmospheric organisms. Not only, according to his new views, was fermentation caused by pre-existing germs of air-borne origin, but each germ induced its own definite specific form of fermentation. Here he fell foul of Béchamp, for according to the latter's physiological explanation, each micro-organism may vary its fermentative effect in conformity with the medium in which it finds itself; may even change in shape, as modern workers are finding out. Pasteur, however, proceeded to label each with a definite and unalterable function. In 1861, claiming to discover a special butyric vibrio, which he thought could live only *without air*, he divided living beings into two classifications, the ærobic and the anærobic, or those that require air and those that flourish without it. Fermentation he defined as life without oxygen. The verdict of time, to which he himself has relegated all scientists for final judgment, is

scarcely in his favour. To quote, for instance, from one of his eulogists in the article on "Fermentation," by Julian Levett Baker, F.I.C., in the *Encyclopædia Britannica*,[1] we read:—"According to Pasteur . . . 'fermentation is life without air, or life without oxygen.' This theory of fermentation was materially modified in 1892 and 1894 by A. J. Brown who described experiments which were in disagreement with Pasteur's dictum."

Pasteur himself, in controversies both with M. Trécul and with the Turin Commission, which investigated his prophylaxis for anthrax, was forced to admit that anærobics could gradually be induced to live with air without becoming ferments and that ærobics could become ferments. Thus he himself destroyed his own classification. Yet this untenable description was Pasteur's chief support for his later equally untenable claim that he had been the first to regard fermentation as a phenomenon of nutrition and of assimilation. In a statement of his made in 1872 and repeated in his *Études sur la Bière*, we find quite contrary teaching[2]:—

"That which separates the chemical phenomenon of fermentation from a crowd of other acts *and especially from the acts of ordinary life* is the fact of the decomposition of a weight of fermentative matter much superior to the weight of the ferment."

What more inevitable act of "ordinary life" could there be than that of nutrition and digestion from which the famous chemist thus separated the phenomenon of fermentation? Pasteur was here only appropriating the same singular idea of physiology that had already been voiced in 1865 by a follower of his, M. Duclaux:—

[3]"When in our alcoholic fermentation we see a certain weight of sugar transformed into alcohol by a weight of yeast one hundred, nay, a thousand times smaller, it is very difficult to believe that this sugar made at any time

[1] Eleventh Edition.

[2] *Comptes Rendus de l'Académie des Sciences* 75, p. 785 (1872).

[3] *Annales Scientiques de l'École Normale*, 2, p. 249 (1865).

a part of the materials of the yeast, and that it (the alcohol) is something like a product of excretion."

It seems strange that scientists should have required the following simple physiological explanation from Professor Béchamp:

[1]"Suppose an adult man to have lived a century, to weigh on an average 60 kilogrammes; he will have consumed in that time, besides other foods, the equivalent of 20,000 kilogrammes of flesh and produced about 800 kilogrammes of urea. Shall it be said that it is impossible to admit that this mass of flesh and of urea could at any moment of his life form part of his being? Just as a man consumes all that food only by repeating the same act a great many times, the yeast cell consumes the great mass of sugar only by constantly assimilating and disassimilating it bit by bit. Now, that which only one man will consume in a century, a sufficient number of men would absorb and form in a day. It is the same with the yeast; the sugar that a small number of cells would only consume in a year, a greater number would destroy in a day; in both cases, the more numerous the individuals, the more rapid the consumption."

By the need of such an explanation evidence is given that Pasteur had failed to understand fermentation to be due to physiological processes of absorption and excretion. It would take too long to follow the varying examples that substantiate this criticism, and, naturally, difficult scientific intricacies were beyond the comprehension of the general public, a great part of whom, having no idea of the processes required for the food they put into their own bodies, were still far less likely even dimly to fathom the nutritive functions of organisms invisible except through the microscope! It was nothing to them that, among the learned Reports of the Academy of Science, treaties were to be found, by a Professor working at Montpellier, that clearly explained the why and the wherefore of the intricate chemical changes that go by the

[1] *Comptes Rendus de l'Académie des Sciences* 75, p. 1523.

name of fermentation. But, on the contrary, more or less everyone had heard, so widely had the subject been ventilated, of the controversy as to whether life, in its lesser forms, sprang invariably from antecedent life, or whether chemical combinations could produce life independently of parents. The public, too, could follow the account of M. Pasteur's holiday tour in pursuit of the question. Very little cudgelling of brains could make anyone understand the history of the flasks that he unsealed, some by a dusty roadside, some on an Alpine summit. Since visible dust could cloud a fluid, it was easy to realise that invisible aerial germs could also affect the contents of the scientist's phials. Minute living things afloat in the atmosphere were not hard to imagine, and Pasteur commenced so enthusiastically to discourse of these that it was not remarkable that an impression was created that he had been the first to demonstrate them; especially since the obstinacy with which a number of scientists declined to endorse his views made him appear a special champion to confound the Sponteparists whose opinions he had cast off so recently.

All this time, in spite of M. Biot's influential patronage, Pasteur had remained outside the select circle of Academicians. But at the end of 1862, as we have said before, he was at last nominated by the Mineralogical Section. No sooner was his candidature commenced than exception began to be taken to his early conclusions on crystallography. None the less, by 36 out of 60 votes, he secured his coveted place in the Academy of Science; and, advised to drop crystallography, he proceeded to experiment further in connection with his new views on air-borne organisms.

To secure matter free from atmospheric dust, he made observations upon muscle, milk, blood, etc., taken from the interior of bodies. From the start, he cannot but have been handicapped by his lack of medical training. His view-point was that of the chemist. According to his conception, as Béchamp points out,[1] the marvellous animal body was likened to wine in the cask or beer in the

[1] *Les Microzymas*, p. 754.

barrel. He looked upon muscle, milk, blood and so forth as mere mixtures of chemical proximate principles. He did, it is true, draw some distinction between the interior of an organism and that of a barrel of beer, or a cask of wine, for we find that he said that the first is[1] "endowed with powers of transformation that boiling destroys" ("*vertus de transformation que l'ébullition détruit*"). Béchamp here shows how Pasteur's mind reverted to the old-fashioned belief in spontaneous alteration. Recognising nothing inherently alive in the composition of animal and vegetable bodies, it was his aim to show that meat, milk, blood, etc., would remain unchanged if completely secured from invasion by aerial organisms. And when, later on, he copied an experiment that Béchamp had undertaken on meat, and found, in his own observation, that, in spite of precautions against germs of the air, the muscular masses of the meat yet became tainted, he was driven to fall back for an explanation upon vague, occult "powers of transformation."

In the same way, for the wonderful evolution of an egg into a bird, he had no solution except these same mysterious transformatory powers. How can it be said that he had destroyed belief in spontaneous generation, when he could only ascribe to a spontaneous change the amazing development of, for instance, the cells of an egg to a circulatory apparatus, bony and nervous systems, glands, organs, and finally a bird covered with feathers? For a spontaneous change there must be if the substance of an egg is only a chemical mixture of the same order as wine or beer. What are Pasteur's "powers of transformation," if not the same as Bonnet's "excellent modification," which produces the organisation of matter, or if not the same as the "*nisus formativus*," or productive forces, vegetable and plastic, with which Needham, and, later Pouchet, the believers in spontaneous generation, were satisfied to explain the phenomenon? Pasteur appears merely to have provided fresh words in place of other words.

[1] *Les Microzymas*, p. 399.

But, here again, such intricacies were beyond the comprehension of the general public. The Man in the Street delved no deeper than the surface test that alterable substances could be preserved by excluding air, and that as the atmosphere was said to be filled with living germs, there was no need to cudgel his brains as to the possible modern emergence of life from mere chemical sources. The religious felt duly grateful for views that appeared to controvert the materialistic tendencies of the nineteenth century, and were blandly innocent of the superficial character of the contradiction. Meanwhile, the talk of the controversy and the exploits of M. Pasteur reached the ears of the Emperor, who, like most rulers, felt it incumbent upon him to patronise contemporary science. Soon after his election to the Academy of Science, M. Pasteur, in the month of March, 1863, had the honour of being presented to Napoleon III at the Tuileries.

As usual his numerous correspondents seem to have been notified at once of the interview, for his son-in-law tells us[1]:—"Pasteur wrote the next day" (to whom he does not say), "I assured the Emperor that all my ambition was to arrive at the knowledge of the causes of putrid and contagious diseases."

Here we have an interesting illustration of the contrast between the methods of Pasteur and Béchamp. As we have seen, right up to 1860, Pasteur's Memoirs contained sponteparist opinions. It was now only 1863. He had but recently changed his standpoint. Yet it is clear that already, before any proofs could have been brought into bearing on the subject, Pasteur, in his mind, was connecting the ferments of the air with a former idea, voiced by earlier workers, Linné, Raspail and others, that specific organisms might be the cause of specific diseases. The best and the worst of us invariably preach against our own individual weaknesses; and therefore Pasteur rightly quoted a great writer as having declared that "The greatest derangement of the mind is to believe things

[1] *The Life of Pasteur*, by René Vallery-Radot, p. 104.

because one wishes them to be so."[1] He could well apprehend this danger, since it was one to which we find he was subject.

Béchamp's attitude to his work was diametrically opposite. He gave his imagination no play until he had interrogated Nature. Not until he had received a direct reply to a direct demand did he allow his mind to be carried away by resultant possibilities, and even then experiments punctuated the course to his conclusions. In short, he did not direct Nature and decide what he wished to discover. He allowed Nature to direct him and made his discoveries follow her revelations.

For fortunate Pasteur, Imperial patronage was no dead letter. Four months after his presentation to Napoleon, in July of the same year, he received direct encouragement from the latter to turn his attention to the vinous diseases that were then interfering with the trade in French wines. Once more Pasteur started on a scientific tour during the holidays, this time to vineyards, and with the Emperor's blessing to lighten his pathway.

Meanwhile, his opponents, Messrs. Pouchet, Joly and Musset, followed his former example and climbed mountains, testing air collected in small glass flasks. They returned triumphant, for although they had scaled 1,000 *mètres* higher than M. Pasteur, there was alteration in their phials.

We have no need here to discuss the wagging of tongues on the subject and M. Flourens' pronouncement in favour of Pasteur at the Academy of Science. It suffices to mention that the deep problem of spontaneous generation became so popular that when Pasteur entered the lecture room of the Sorbonne on the evening of April 7th, 1864, to discourse on the subject, every seat available was filled, not simply by learned professors, but also by literary celebrities, Alexandre Dumas and George Sand among them, and also Princesse Mathilde and all the well-known votaries of fashion, the "smart set" of Paris. And happily

[1] *Comptes Rendus de l'Académie des Sciences* 80, p. 91 (1875).

for these worldlings, M. Pasteur had nothing very abstruse to set before them. He simply asseverated the impossibility of dispensing with parents, a subject likely to provoke banter rather than very deep reasoning. He wound up by explaining an experiment in which dust from the air had been excluded from a putrescible liquid and in consequence no animalculæ had become apparent.

To quote his own words:[1] "It is dumb because I have kept it from the only thing man cannot produce, from the germs that float in the air, from Life, for Life is a germ and a germ is Life. Never will the doctrine of spontaneous generation recover from the mortal blow of this simple experiment."

There was never a word how this partial truth had been originally arrived at years before, so far back as 1857, by his contemporary, Professor Béchamp. There was no acknowledgment made of the great Memoir that had enlightened Pasteur's progress and revealed to him early errors. He took to himself all the credit, and that which is taken sufficiently forcibly, the public seldom tries to hold back. We can picture the fashionable audience dispersing, proud of having understood the subject under discussion, as they no doubt imagined, and delighted with the lecturer for having proved them so much more scientific and clever than they had ever supposed themselves. Pasteur became the protégé of Society; the Church gave him her blessing; the Emperor invited him at the end of 1865 to spend a week at the Palace of Compiègne. His name and fame were established. Can we wonder that scientists who had never received such honours should have felt reluctant to oppose this favourite of fortune, who was naturally singled out to undertake scientific missions.

But, to pause for an instant and consider his noted lecture at the Sorbonne, what after all was there in it? He had merely ascribed to the germs of the air a mysterious quality—life—that he denied to the component parts of more complicated animal and vegetable beings. For the

[1] *The Life of Pasteur*, by René Vallery-Radot, p. 109.

origin, the source of his atmospheric germs, he provided no explanation, neither has any since been found by his innumerable followers, for whom the description "Life is a germ and a germ is Life" was soon to evolve into "Disease is a germ and a germ is a disease," an infinitely more lugubrious axiom.

Was Pasteur correct even in his denial of alteration apart from air-borne organisms? In his own experiment upon meat, he had to admit that the latter became tainted. To assume that this was caused by some faultiness in operation is not to explain the appearance of micro-organisms in cases where no air-borne germs could possibly account for their origin. Thus it is that Pasteur's boast in his lecture at having struck a "mortal blow" at the doctrine of spontaneous generation has not met with real fulfilment. Not only was his contemporary, Pouchet, never satisfied, but the later work of M. Gustave le Bon and of Dr. Charlton Bastian affected to demonstrate, according to their view, the production of organised beings from inorganic matter.

Professor Bastian asserts:—[1]"Living matter may have been continuously coming into being all over the surface of the earth ever since the time of man's first appearance upon it; and yet the fact that no member of the human race has ever seen (or is ever likely to see) such a birth throws no doubt upon the probability of its occurrence."

Professor Bastian based this belief upon such observations as his experiment with the "*cyclops quadricornis*, one of the *Entomostraca* so commonly to be found in ponds."[2]

"If we take one of these little creatures," he writes, "put it in a drop of distilled water, on a glass slip with a fragment of a No. 2 cover-glass on each side of it, and place over all a cover-glass, it will be found that the animal is soon killed by the weight of the latter, though the fragments of glass prevent rupture of the body. We may then

[1] *The Evolution of Life*, by H. Charlton Bastian, M.A., M.D., F.R.S., F.L.S. p. 31.

[2] *The Nature and Origin of Living Matter*, by H. Charlton Bastian, M.A., M.D., F.R.S., F.L.S., R.P.A. ed. p. 110. (Watts and Co.).

place the microscope slip in a Petri dish containing a thin stratum of water (so as to prevent evaporation from beneath the cover-glass) and fixing upon one of the tail *setae* (these being larger than those of the abdominal feet), we may examine it from time to time. What may be observed is this. After an interval of two or three days (the duration depending upon the temperature of the air at the time) we may see, under a high power of our microscope, scarcely visible motionless specks gradually appear in increasing numbers in the midst of the structureless protoplasm, and, still later, we may see some of these specks growing into bacteria. . . . At last the whole interior of the spine becomes filled with distinct bacteria. . . . Later still, all the bacteria, previously motionless, begin to show active swarming movement. In such a case it is clear we have to do with no process of infection from without, but with a *de novo* origin of bacteria from the protoplasmic contents of the spines or *setae*. The fact that they appear in these situations as mere separate motionless specks, and gradually take on the forms of bacteria (also motionless at first) is, as I have previously indicated, just what we might expect if they had actually taken origin in the places where they appear. On the other hand, such a mode of appearance is totally opposed to what might be expected if the micro-organisms had obtained an entry from without, through the tough chitinous envelope of the spines."

Professor Bastian gives numerous examples of the finding of bacteria in internal animal organs and in fruit and vegetables, where he demonstrates the impossibility of an invasion. Can the followers of Pasteur provide any solution of the mystery? If they cannot, it must be conceded that no "mortal blow" at the doctrine of spontaneous generation was struck by Pasteur, as he proudly boasted. The dealer of the blow, or, at any rate, the provider of an explanation, apart from heterogenesis, was not the French chemist, dilating to a fashionable audience which included "all Paris," but a hard-working French Professor and physician, who was also a chemist and a

naturalist, and who was taking little part in all the talk because he was so hard at work wresting fresh secrets from Nature.

Even admitting that he demonstrated before Pasteur, and far more thoroughly, the rôle of air-borne organisms, it may yet be asked how Béchamp's observations enlightened any better the deeper depths of the heterogenetic mystery.

The answer to this is that, in his Memoir of 1857, the Professor did not include certain of his observations. His reason for the omission was that the results he obtained seemed too contradictory to be accurate. Believing that he had made some mistake, he set aside these particular experiments for the time being. In the end, as the following pages hope to set forth, his apparent failure was to prove the solution of the problem and was to give, so he at least believed, the basic explanation of the development of organised life from the minutest commencements. It was, in fact, according to him, to be the nearest elucidation ever given of animal and vegetable upbuilding, of the processes of health, disease and final disruption. In short, it was to wrest from Nature the stupendous truth, which, in the great Master's own words, rings out like a clarion:—"*Rien n'est la proie de la mort; tout est la proie de la vie!*"[1]

[1] "Nothing is the prey of death; everything is the prey of life!"

PART TWO

THE MICROZYMAS

CHAPTER VIII

The "Little Bodies"

Just as certain musicians seem born with a natural facility for a particular instrument, so in the world of science, from time to time, men arise who appear specially gifted in the use of technical intruments. It was, no doubt, Professor Béchamp's extraordinary proficiency as a microscopist, as well as the insight of genius, that enabled him from the start of his work to observe so much that other workers ignored when employing the microscope; while his inventive brain led to an application of the polarimeter which greatly assisted him. His powers combined in a remarkable degree the practical and theoretical. Instead of failing, like many men of big brain capacity, when manual dexterity was needed, the Professor's deft fingers and keen-sighted eyes were ever the agile assistants of his mighty intellect.

From the time of his earliest observations, he was quick to notice minute microscopic objects, much smaller in size than the cells of the organisms he examined. He was by no means the first to observe these; others had done so before him; but although they applied to them such names as "scintillating corpuscles," "molecular granulations" and so forth, no one was much the wiser as to their status and function. Most of what had been said about them was summed up in Charles Robin's definition in the *Dictionary of Medicine and Surgery* (1858), in which he described the minuteness of "very small granulations formed of organised substance" found in the tissues, cells,

fibres and other anatomical elements of the body, and in great abundance in tuberculous substances and other disease matters.

Béchamp, always so careful to avoid unsubstantiated conclusions, did not allow his imagination to run away in regard to them. He at first merely noted them and bestowed upon them the noncommittal name of "little bodies." He had no further enlightenment in regard to them at the time when his new duties took him to Montpellier and he there brought to a close the observations that he had commenced at Strasbourg and which he recounted and explained in his Memoir of 1857.

It will be remembered that for many of these experiments, the Professor employed various salts, including potassium carbonate, in the presence of which the inversion of cane-sugar did not take place, in spite of the absence of creosote. Another experiment that he made was to substitute for potassium carbonate, calcium carbonate in the form of chalk. Great was his surprise to find that in spite of the addition of creosote, to prevent the intrusion of atmospheric germs, cane-sugar none the less underwent inversion, or change of some sort. In regard to creosote, Béchamp had already proved that though it was a preventive against the invasion of extraneous organisms, it had no effect in hampering the development of moulds that were already established in the medium. The experiments in which he had included chalk seemed, however, to contradict this conclusion, for in these cases, creosote proved incapable of preventing the inversion of sugar. He could only believe that the contradiction arose from some faultiness of procedure; so he determined to probe further into the mystery and meanwhile to omit from his Memoir any reference to the experiments in which chalk had proved a disturbing factor.

The work that Professor Béchamp undertook in this connection is an object lesson in painstaking research. To begin with, he had, first, chalk and, afterwards, a block of limestone conveyed to his laboratory with great precau-

tions against any air coming into contact. To continue, he proved by innumerable experiments that when all access of air was entirely shut away, no change took place in a sugar solution, even when chemically pure calcium carbonate, $CaCO_3$, was added, but directly ordinary chalk, even from his specially conserved block was introduced, fermentation took place, although the entry of atmospheric germs had been guarded against completely. No addition of creosote, even in increased doses, could then prevent the inversion of the sugar.

Béchamp was, naturally, extremely surprised to find that a mineral, a rock, could thus play the part of a ferment. It was clear to him that chalk must contain something over and above calcium carbonate. He, therefore, called to his help his good ally, the microscope. Working with the highest power obtainable, he undertook a minute investigation both of pure calcium carbonate and of the chalk he had used for his experiments. Great was his amazement to find in the latter "little bodies," similar to those he had noted in other observations, while nothing of the sort was to be seen in the former. Also, while in the microscopic preparation of the calcium carbonate everything was opaque and motionless, in that of the chalk the "little bodies" were agitated by a movement similar to that known as "Brownian," after the naturalist, Robert Brown, but which Béchamp differentiated from it.[1] These "little bodies" were distinguishable by the way in which they refracted light from their opaque surroundings. They were smaller than any of the microphytes seen up to that time in fermentations; but were more powerful as ferments than any known and it was because of their fermentative activity that he regarded them as living.

To form any correct estimate of the magnitude of the discovery upon the brink of which Béchamp hovered, we must remind ourselves of the scientific opinions of the epoch. The Professor's observations were made at a date when most believed in Virchow's view of the cell as the

[1] *La Théorie du Microzyma, par A. Béchamp*, p. 115.

unit of life in all forms, vegetable and animal, and sponteparist opinions were held by a large body of experimenters, including, at that time, Pasteur. In the midst of this confusion of ideas, Béchamp clung firmly to two axioms:—Firstly, that no chemical change takes place without a provocative cause. Secondly, that there is no spontaneous generation of any living organism. Meanwhile, he concentrated his mind upon the "little bodies."

He realised, at the start, that if those he had discovered in chalk were really organised beings, with a separate independent life of their own, he ought to be able to isolate them, prove them to be insoluble in water, and find them composed of organic matter. He succeeded in isolating them and proved carbon, hydrogen, etc., to be their component parts and demonstrated their insolubility. If they were living beings, it followed that it must be possible to kill them. Here, again, he found the truth of his contention, for when he heated chalk, together with a little water, to 300° C. (572° F.), he afterwards proved it to have become devoid of its former fermentative power. The "little bodies" were now quite devoid of the movement that before had characterised them. Among other points, he discovered that if, during the process of fermentation by these minute organisms, all foreign invasions were guarded against by rigid precautions, the little bodies increased and multiplied. This observation was to stand him in good stead in his subsequent[1] researches.

Béchamp observed that the chalk he had used seemed to be formed mostly of the mineral remains of a microscopic world, long since vanished, which fossil-remains, according to Ehrenberg, belong to two species, called *Polythalamis* and *Nautilæ* and which are so minute that more than two millions would be found in a piece of chalk weighing one hundred *grammes*. But, over and above these remains of extinct beings, the Professor saw that the white chalk con-

[1] *La Théorie du Microzyma, par A. Béchamp,* pp. 113, 114.

tains organisms of infinitesimal size, which according to him, are living and which he imagined might possibly be of immense antiquity. The block of limestone he had obtained was so old that it belonged to the upper lacustrian chalk formation of the Tertiary Period; yet he proved it to be possessed of wonderful fermentative properties, which he satisfied himself to be due to the presence of the same "little bodies."[1]

He continued a persistent examination of various calcareous deposits and not only found the same minute organisms, but discovered them to possess varying powers of causing fermentation. The calcareous tufa and the coal areas of Bessège had very little power either to liquefy starch or to invert cane-sugar; while the peat-bogs, on the other hand, and the waste moors of the Cévennes, as well as the dust of large cities, he proved to contain "little bodies" possessing great powers for inducing fermentation. He continued his investigations and found them in mineral waters, in cultivated land, where he saw that they would play no inconsiderable rôle, and he believed them to be in the sediment of old wines. In the slime of marshes, where the decomposition of organic matter is in progress, he found the "little bodies" in the midst of other inferior organisms, and, finding also alcohol and acetic acid, attributed to these minute living beings the power that effects the setting free of marsh-gas.

Nature having confided such wonderful revelations, the time had come for Professor Béchamp to allow his mind to interpret their meaning. The experiments he had omitted from his great Memoir, instead of being faulty, now seemed to hold marvellous suggestions. The "little bodies" he had discovered in the chalk appeared to be identical with the "little bodies" he had observed in the cells of yeast and in the body-cells of plants and animals, the "little bodies" that, for the most part, went by the name of "molecular granulations." He remembered that Henle had, in a vague way, considered these granulations to be

[1] *Les Microzymas, par A. Béchamp,* pp. 940, 944.

structured and to be the builders of cells; and Béchamp saw that if this were true, Virchow's theory of the cell as the unit of life would be shattered completely. The granulations, the "little bodies," would be the anatomical elements, and those found in the limestone and chalk he believed might even be the living remains of animal and vegetable forms of past ages. These must be the upbuilders of plant and animal bodies and these might survive when such corporate bodies have long since undergone disruption.

At this point we may draw attention to the cautiousness of Béchamp's proceedings. Although his investigations of chalk were commenced at the time of the publication of his Beacon Memoir, he continued to work at the subject for nearly ten years before giving publicity to his new observations. Meanwhile, the proverb about the ill wind was exemplified in his case, for diseases affecting vines were becoming the scourge of France, and led him to undertake some experiments that were helpful in widening the new views that he was gradually formulating.

We have already seen how in 1863 M. Pasteur had been despatched with the Emperor's blessing to investigate the troubles of French wine-growers. There was no official request for Professor Béchamp's assistance, but, none the less, with his unfailing interest in all scientific problems, he started to probe into the matter, and in 1862, a year before Pasteur, began his researches in the vineyard.

He exposed to contact with air, at the same time and place (1) grape-must, decolourised by animal charcoal, (2) grape-must, simply filtered, and (3) grape-must, not filtered. The three preparations fermented, but to a degree in an inverse order from the above enumeration. Further, the moulds, or ferments, that developed were not identical in the three experiments.

The question thus arose: "Why, the chemical medium being the same in the three cases, did it not act in the same manner upon the three musts?"

To solve the riddle, the Professor instituted more experi-

ments. Whole healthy grapes, with their stalks attached, were introduced direct from the vine into boiled sweetened water, cooled in a current of carbonic acid gas, while the gas still bubbled into the liquid. Fermentation took place and was completed in this medium, preserved during the whole process from the influence of air. The same experiment succeeded when the grapes were introduced into must, filtered, heated and creosoted.

From these researches it was evident that neither oxygen nor air-borne organisms were the cause of the fermentation, but that the grape carried with it the provocative agents.

Professor Béchamp communicated the results of his experiments to the Academy of Science in 1864, and among its Reports the subject may be found exhaustively treated.[1] He had come to the conclusion that the agent that causes the must to ferment is a mould that comes from the outside of the grape, and that the stalks of grapes and the leaves of vines bear organisms capable of causing both sugar and must to ferment; moreover, that the ferments borne on the leaves and stalks are sometimes of a kind to injure the vintage.

The year 1864, when Béchamp presented this Memoir, marks an era in the history of biological research, for, on the 4th April, of that self-same year, he read before the Academy of Science, his explanation of the phenomena of fermentation. He showed the latter to be due to the processes of nutrition of living organisms, that absorption takes place, followed by assimilation and excretion and for the first time used the word *zymase* to designate a soluble ferment.

It was the following year that M. Duclaux, a pupil of Pasteur's, tried to cast scorn upon Béchamp's illuminating explanation, thus supplying documentary proof that his master had no right to lay claim to having been a pioneer of this teaching.

Béchamp, who, in 1857, had so conclusively proved air-

[1] *Comptes Rendus* 59, p. 626.

borne organisms to be agents of fermentation, now, in 1864, equally clearly set forth the manner in which the phenomenon is induced. All the while he was at work on Nature's further mysteries, undertaking experiments upon milk in addition to many others, and in December of the same year informed M. Dumas of his discovery of living organisms in chalk. Later, on the 26th September, 1865, he wrote to M. Dumas on the subject and, by the latter's request, his letter was published the next month in the *Annales de Chimie et de Physique*.[1]

Here he stated:—"Chalk and milk contain living beings already developed, which fact, observed by itself, is proved by this other fact that creosote, employed in a non-coagulating dose, does not prevent milk from finally turning, nor chalk, without extraneous help, from converting both sugar and starch into alcohol and then into acetic acid, tartaric acid and butyric acid."

Thus we clearly see the meaning in every single experiment of Béchamp's and the relation that each bore to the other. His rigid experiments with creosote made it possible for him to establish further conclusions. Since creosote prevented the invasion of extraneous life, living organisms must be pre-existent in chalk and milk before the addition of creosote. These living organisms were the "little bodies" that he had seen associated in cells and singly in the tissues and fibres of plants and animals. Too minute to differentiate through the microscope, Béchamp tells us[2] that—"The naturalist will not be able to distinguish them by a description; but the chemist and also the physiologist will characterise them by their function."

He was thus not checked in his investigations by the minuteness of his objects of research, so infinitesimal as, in many cases, no doubt, to be ultra-microscopic. Neither was he disturbed by the ridicule with which many of his contemporaries received his account of the little bodies in chalk and milk. Being a doctor, he was much helped in

[1] *4e série*, 6, p. 248.

[2] *La Théorie du Microzyma, par A. Béchamp*, p. 124.

his research work by his medical studies. In the year 1865 he found in fermented urine that, besides other minute organisms, there were little bodies so infinitesimal as to be only visible by a very high power of the microscope, obj. 7, oc. I, Nachet. He soon after found these same little bodies in normal urine.

The following year, 1866, he sent up to the Academy of Science a Memoir entitled "On the Rôle of Chalk in Butyric and Lactic Fermentations and the Living Organisms Contained in It."[1]

Here he detailed experiments and proposed for the "little bodies" the name of *microzyma*, from Greek words that mean "small" and "ferment." This very descriptive nomenclature portrayed them as ferments of the minutest perceptible order.

To the special little bodies found in chalk, he gave the name of *microzyma cretæ*.

Without loss of time, he continued his investigations on the relation of the microzymas of chalk to the molecular granulations of animal and vegetable cells and tissues, and also made numerous further geological examinations. The results of the latter were partly incorporated in a Memoir "On Geological Microzymas of Various Origin," an extract of which was published among the Reports of the Academy of Science.[2]

In this he asks:—"What is now the geological significance of these microzymas and what is their origin?" He answers:—"I believe that they are the organised and yet living remains of beings that lived in long past ages. I find proof of this both in these researches and in those that I have carried out by myself and in collaboration with M. Estor on the microzymas of actual living beings. These microzymas are morphologically identical, and, even though there may be some slight differences in their activity as ferments, all the components that are formed under their influence are, nevertheless, of the same order.

[1] *Comptes Rendus* 63, p. 451. *Les Microzymas, par A. Béchamp*, p. 940.

[2] *Comptes Rendus* 70, p. 914. *Les Microzymas, par A. Béchamp*, p. 944.

Perhaps one day geology, chemistry and physiology will join in affirming that the great analogies that there are stated to be between geological fauna and flora and living fauna and flora, from the point of view of form, exist also from the point of view of histology and physiology. I have already set forth some differences between geological microzymas of various origin: thus, while bacteria may appear with the limestone of Armissan and that of Barbentane, these are never developed in the case of chalk or of Oolithic limestone under the same circumstances. Analogous differences may be met with among the microzymas of living beings. . . . It is remarkable that the microzymas of limestones that I have examined are almost without action at low temperatures, and that their activity only develops between 35 and 40 degrees. A glacial temperature, comparable to that of the valley of Obi, would completely arrest this activity."

Though many ridiculed such new and startlingly original ideas and though many nowadays may continue to do so, we have to remember that the mysteries of chalk may well bear much more investigation. Modern geologists seem ready to admit that chalk possesses some remarkable qualities, that under certain conditions it produces movements that might evidence life and induce something like fermentation. Professor Bastian, though his inferences differ completely from Béchamp's, again confirms the latter's researches. We read in *The Origin of Life*[1] as follows:—"We may, therefore, well recognise that the lower the forms of life—the nearer they are to their source—the greater is likely to have been the similarity among those that have been produced in different ages, just as the lowest forms are now practically similar in all regions of the earth. How, otherwise, consistently with the doctrine of evolution, are we to account for the fact that different kinds of bacilli and micrococci have been found in animal and vegetable remains in the Triassic and

[1] *The Origin of Life*, by H. Charlton Bastian, M.A., M.D., F.R.S., F.L.S. pp. 67, 68.

Permian strata, in Carboniferous limestone and even as low as the Upper Devonian strata? (See *Ann. des Sciences Nat.* (*Bot.*), *1896*, *II*, *pp. 275-349.*) Is it conceivable that with mere lineal descent such variable living things could retain the same primitive forms through all these changing ages? Is it not far simpler and more probable to suppose, especially in the light of the experimental evidence now adduced, that instead of having to do with unbroken descent from ancestors through these aeons of time, as Darwin taught, and is commonly believed, we have to do, in the case of Bacteria and their allies, with successive new births of such organisms throughout these ages as primordial forms of life, compelled by their different but constantly recurring molecular constitutions to take such and such recurring forms and properties, just as would be the case with successive new births of different kinds of crystals?"

We have introduced this quotation merely to show the confirmation by Bastian of Béchamp's discovery of living elements in chalk and limestone, and must leave to geologists to determine whether infiltration or other extraneous sources do or do not account for the phenomena. If they do not, we might be driven to believe in Professor Bastian's explanation of successively recurring new births of chemical origin, were it not for Professor Béchamp's elucidation of all organised beings taking their rise from the microzymas, which we may identify with what are now known as microsomes when found in cells, whether animal or vegetable. Thus we see that Béchamp's teaching can explain appearances which, without it, can only be accounted for by spontaneous generation, as shown by Professor Bastian. Whether Béchamp were correct in his belief that the microzymas in chalk are the living remains of dead beings of long past ages is not a point that we care to elaborate. We wish to leave the subject of chalk to those qualified to deal with it and have only touched on it here because these initial observations of Professor Béchamp's were what led to his views of the cell, since

confirmed by modern cytology, and to what may be termed his microzymian doctrine, which we are inclined to believe has been too much neglected by the modern school of medicine. Those disposed to ridicule Béchamp may well ponder the fact that the first word rather than the last is all that has been said about micro-organisms. For instance, it is now claimed that in the same manner that coral is derived from certain minute sea-insects, so particular micro-organisms not only aid in the decomposition of rocks and in the formation of chalk and limestone, but play an active part in the forming of iron deposits.[1]

Though, as we have said, derided by some, Béchamp's work at this time was beginning to attract a great amount of attention, and mid-way through the sixties of the last century, it gained for him an enthusiastic co-partner in his labours. This was Professor Estor, physician and surgeon in the service of the hospital at Montpellier, and who, besides being in the full swing of practical work, was a man thoroughly accustomed to research and abundantly versed in scientific theories. He had been astounded by

[1] Attention has been drawn to a remarkable and up-to-date parallel of Béchamp's discovery of microzymas in chalk. See *The Iron and Coal Trades Review* for May 4th, 1923. In this, in an article on *Coal Miners' Nystagmus*, Dr. Frederick Robson puts forward a statement by Professor Potter "that there are in coal bacteria capable of producing gases, and that the gases isolated are methane, carbon dioxide and carbon monoxide, with heating up to 2 deg. C. (35 deg.-36 deg. F.). It would appear as if wood were capable of containing in its metamorphosed state (coal) the bacteria originally present in the tree stage of its existence. It is possible, too, that different kinds of orders of flora would give rise to the presence of different species of bacteria . . . possibly resident in the woody-fibred coal. . . . This idea of bacterial invasion of coal suggests that some degree of oxidation may be due to the great army of ærobic or anærobic bacteria which may give rise to oxidation and may be the genesis of coal gases in the pits, *i.e.*, that oxidation is due to living organisms with increase of 2 deg. C. of heat. This has been disproved, but it is evident that bacteria exist. . . . There is evidence to show that at 100 deg. C. (212 deg. F.) all bacterial action ceases. If soft coals and bacterial invasion go hand in hand, in some kind of relationship, then as the coal measures become harder from east to west, the microbic invasion or content may diminish with the ratio of gaseous liberation."

Thus more modern corroboration is found of Béchamp's astounding discovery; while it is due to him alone that we may understand the origin of the so-called bacteria. According to his teaching, these must be the surviving microzymas, or microsomes, of the cells of pre-historic trees, known to us now in their fossilized form as coal, but still preserving intact the infinitesimal lives that once built up primeval vegetation.

the discoveries of Professor Béchamp, which he described as laying the foundation stone of cellular physiology. In 1865 he published in the *Messager du Midi* an article that placed in great prominence Béchamp's explanation of fermentation as an act of cellular nutrition. This conception made a sensation in Germany, for while, in a sense, confirming Virchow's cellular doctrine, it showed the German scientist's view to be only partial.

Béchamp's star was, perhaps, just now at its zenith. Conscious that his great discovery, as he proceeded with it, would illumine the processes of life and death as never before in the course of medical history, he was also happy in finding a zealous coadjutor, who was to share in his work with persistence and loyalty, while, at the same time, a little band of pupils arose full of eagerness to forward their great Master's researches. Indistinguishable in the distance loomed the tiny cloud that on gathering was to darken his horizon. France was in trouble. Her whole silk industry was threatened by mysterious diseases among silk-worms. Unsolicited, and unassisted pecuniarily, Béchamp at once turned his mind to the problem, not knowing when he did so that it was to bring him into direct rivalry with the man who had been appointed officially, and that, while providing the latter with solutions to the enigma, no gratitude was to be his, but, instead, the undying hatred and jealousy of Fortune's favourite, Louis Pasteur!

CHAPTER IX

Diseases of Silk-Worms

At the commencement of the year 1865, the epidemic among silk-worms had become so acute that the sericultural industry of France was seriously threatened. Eggs, worms, chrysalides and moths were all liable to be affected. The trouble was characterised by the presence of a microscopic object called the "vibrant corpuscle," or "Corpuscle of Cornalia," after the scientist who first observed it; while the malady became popularly known as "*pébrine*," from the patois word *pébré*, pepper.

It appears to have been through the advocacy of M. Dumas that M. Pasteur was appointed by the Minister of Agriculture to investigate the matter, and no one can have attended a popular lecture on the subject without having been informed that Pasteur's work redeemed for his country more money than the war indemnity wrung from France by the Germans after 1870. What really happened was that Pasteur's luck stood him in extraordinarily good stead. Had Professor Béchamp not provided him with the elucidation of the silk-worm mystery, a very different story might have been told.

Nothing better illustrates the remarkable acuteness of Béchamp's intellect than the rapidity with which he solved the cause of *pébrine* and suggested a preventive. Although he was entirely unassisted and obliged to defray any entailed expenses out of his own pocket, already in the year 1865, he was able to state before the Agricultural Society of Hérault that *pébrine* was a parasitical disease and that creosote could be used to prevent the attack of the parasite.

Meantime, however, M. Pasteur had been intrusted by the Government with an investigation, and no one who understands anything of departmental red tape will wonder that, instead of at once accepting Béchamp's

verdict, Agricultural Societies waited to hear the pronouncement of the official representative. Plenty of patience had to be exercised.

M. Pasteur arrived on his mission at Alais in June, 1865, having, as he stated before long in his Note to the Academy of Science,[1] "no serious title" to his fresh employment, owing to his ignorance of the subject. "I have never even touched a silk-worm," he had written previously to M. Dumas, and the perusal of an essay on the history of the worm by Quatrefages comprised his study up to June, 1865.

Yet, as some statement was expected from him, he managed to address a Communication to the Academy of Science on the 25th September of the same year in which he gave vent to the following extraordinary description[2]:—"The corpuscles are neither animal nor vegetable, but bodies more or less analogous to cancerous cells or those of pulmonary tuberculosis. From the point of view of a methodic classification, they should rather be ranged beside globules of pus, or globules of blood, or, better still, granules of starch than beside infusoria or moulds. They do not appear to me to be free, as many authors think, in the body of the animal, but well contained in the cells. . . . It is the chrysalide, rather than the worm, that one should try to submit to proper remedies."

One may well imagine that such a description evoked ridicule from Professor Béchamp, who scornfully wrote.[3]—"Thus this chemist, who is occupying himself with fermentation, has not begun to decide whether or no he is dealing with a ferment."

What Pasteur had done, however, was to give a detailed description that was wrong in every particular. There for a considerable time he left the matter, while the deaths of his father and two of his daughters intervened, and he received the honour of being invited as a guest to spend

[1] *Comptes Rendus* 61, p. 506.
[2] *C. R.* 61, p. 506.
[3] *Les Grands Problèmes Médicaux, par A. Béchamp*, p. 7.

a week with the Emperor and Empress at the Palace of Compiègne.

Napoleon III was, we are told, deeply interested in science. At any rate, he and the Empress listened with condescending deference to Pasteur's discourses. The latter was not only brought into close contact with eminent diplomatists and the shining lights of art and literature, but was singled out from among these celebrities for special Imperial favours. His silk-worm perplexities were confided to Eugénie, and that gracious lady encouraged him to fresh endeavours. Limelight is invariably thrown upon those smiled upon by Imperial personages, and it is easy to understand the increasing deference that began to be shown to Pasteur by most of his compeers. As regards the silk-worm disease, instead of being watchful for the correct verdict, the world at large merely waited to hear what M. Pasteur had to say on the subject.

In February, 1866, the latter again started for that part of France then suffering from the trouble, and this time fortified himself with the company of scientific assistants. Government again gave all the help possible, and the Minister of Public Instruction granted special leave of absence to M. Gernez, a Professor at the College Louis le Grand, so that he might be free to help Pasteur. Yet, in spite of all this assistance, and notwithstanding extra early rising, his biographer has to admit that the results Pasteur arrived at "were being much criticised."[1] His actual pronouncements his son-in-law has wisely passed over and instead has introduced various topics to divert the attention of the reader who persists in asking—"What was Pasteur's solution of the silk-worm mystery?"

Fortunately, lovers of truth can find the exact answers in the Reports of the French Academy of Science. The first one to turn to however, is a Note, not by M. Pasteur, but by Professor Béchamp, which comes under the date of the 18th June, 1866.[2]

[1] *The Life of Pasteur*, by René Vallery-Radot, p. 133.

[2] *Comptes Rendus* 62, p. 1341.

In the midst of his strenuous professorial duties and his constant researches in other directions, Béchamp snatched time to send up to the Academy fuller details of the disease *pébrine* and measures for preventing it. His note was entitled "On the Harmlessness of the Vapours of Creosote in the Rearing of Silk-Worms." He repeated the pronouncement he had made the previous year and clearly stated:—"The disease is parasitical. *Pébrine* attacks the worms at the start from the outside and the germs of the parasite come from the air. The disease, in a word, is not primarily constitutional." He went on to explain how he developed the eggs, or, as they are called, the seeds, of the silk-worms in an enclosure in which the odour of creosote was produced from a very minute dose of the drug. The eggs, thus hatched, were all free from *pébrine*. As Professor Béchamp never committed himself to statements until he had proof positive, we find in this verdict upon *pébrine* the decisive clearness that characterises all his opinions.

Pasteur was still so much in the dark that he had not even the acumen to gauge the correctness of the views of the great teacher of Montpellier. But this Note of Béchamp's was, no doubt, a trial to him. Here was another worker pronouncing upon a subject that had been officially relegated to him and hallowed by the blessing of the beautiful Empress. Accordingly, on the 23rd July, 1866, Pasteur unburdened himself of a Statement to the Academy of Science on the Nature of *Pébrine*.[1] It was entitled "New Studies on the Disease of Silk-Worms." And here we must look for the great discovery said to have been provided by Pasteur for "the salvation of sericulture." It was this:—"The healthy moth is the moth free from corpuscles; the healthy seed is that derived from moths without corpuscles." Such an obvious conclusion is laughable! Still, as it could not be condemned as incorrect, it would have been as well for Pasteur to have ventured no further. Instead, he proceeded: "I am very much inclined to believe that there is no actual disease of silk-worms. I cannot

[1] *Comptes Rendus* 63, p. 126-142.

better make clear my opinion of silk-worm disease than by comparing it to the effects of pulmonary phthisis. My observations of this year have fortified me in the opinion that these little organisms are neither animalcules nor cryptogamic plants. It appears to me that it is chiefly the cellular tissue of all the organs that is transformed into corpuscles or produces them." Not a single proof did he bring forward of a fact that would, if true, have been marvellous: not a single suggestion did he give of any experiment to determine the asserted absence of life in the corpuscles or their relation to the disease. Finally he went out of his way to contradict Béchamp and in so doing set a definite seal on his blunder. "One would be tempted to believe, especially from the resemblance of the corpuscles to the spores of mucorina, that a parasite had invaded the nurseries. *That would be an error.*"

This intentional dig at another worker was singularly unlucky, for it provides proof positive of the lie direct given by Pasteur to a correct solution to which he afterwards laid claim. Here was the man who had so utterly renounced his former sponteparist views as to ascribe all fermentative effects, all vital phenomena, to air-borne causes, now denying the extraneous origin of a disease that was proved by Béchamp to be undoubtedly parasitic.

The latter at once fortified his conclusions by an account of the experiments upon which he had based them. On the 13th August, 1866, he presented a Note to the Academy of Science: "Researches on the Nature of the Actual Disease of Silk-Worms."[1] In this he described a process of washing the seeds and worms, which gave proof that those affected had been invaded by a parasite. In answer to M. Pasteur, he declared that the vibrant corpuscle "Is not a pathological production, something analogous to a globule of pus, or a cancer cell, or to pulmonary tubercles, but is distinctly a cell of a vegetable nature."

Again, on the 27th August, another Note to the

[1] *Comptes Rendus* 63, p. 311.

Academy[1] described experiments that proved the vibrant corpuscle to be an organised ferment.

Later, on the 4th February of the following year, 1867, a fresh Memoir, sent to the Academy,[2] detailed more experiments that not only showed the corpuscle to be a ferment, but also that after the inversion of sugar, fermentation went on, producing alcohol, acetic acid, and another non-volatile acid.

In January, 1867, Pasteur, who had been away, returned to Alais, apparently at last enlightened by Professor Béchamp's explanations. In a letter to M. Duruy, the Minister of Public Instruction, he seems to have started to take to himself credit for solving the mystery of the silk-worm trouble. This would account for the almost pathetic plea put forward by Béchamp for a recognition of his outstanding priority in providing a correct scientific explanation.

The latter now, on the 29th April, 1867, provided the Academy of Science[3] with an even fuller account, in which he stated his opinion that the vibrant corpuscle was a spore, and demonstrated that it multiplied in an infusion of dead worms, chrysalides and moths, and that creosote diminished this multiplication. He added to this Note a plate of designs of the microscopic examination of this reproduction of corpuscles. "Thus," he said, "is completed the parasitic theory of *pébrine* for the triumph of which I have struggled for nearly two years. I venture to hope that the priority of the idea and of the experiments that have demonstrated it will not be disputed." He showed that up to the previous August he had been alone in holding his opinion, with the exception of M. Le Ricque de Monchy, to whom he expressed gratitude for his encouragement and able assistance.

Alas for Béchamp! Pasteur was unhappily devoid of a similar habit of rendering due honour. Convinced against

[1] *Comptes Rendus* 63, p. 391.
[2] *C. R.* 64, p. 231.
[3] *C. R.* 64, p. 873.

his will by the Professor's irrefutable proofs, there was nothing for him but to turn a complete *volte face*, as he had done before when Béchamp incontestably proved the erroneousness of belief in spontaneous generation.

On the self-same 29th April, 1867, we find among the Reports of the Academy of Science[1] a letter from Pasteur to Dumas, dated Alais, 24th April. In this Pasteur feebly excused his mistake on the score that he had held his erroneous view in good company with "many persons of great repute," and he also pleaded the impossibility of recognising the mode of reproduction of the corpuscles. Instead of any acknowledgement to Professor Béchamp for his full illuminating revelations, Pasteur coolly expressed a hope that he himself would soon be able to present an almost complete study of the disease. His omission to do so then and there seems a noteworthy proof of a continued want of clear understanding.

We find among the Reports[2] of the 20th May, 1867, a letter addressed to the President of the Academy of Science by Béchamp, dated the 13th May, on the subject of Pasteur's Communication of the previous April. He pointed out the error of Pasteur's former views and vindicated his own priority in discovering the true nature of the corpuscles and their mode of reproduction.

On the same date, he brought forward[3]—"New Facts to Help the History of the Actual Disease of Silk-Worms and the Nature of the Vibrant Corpuscle." Here he claimed that the corpuscles were air-borne and to be found on mulberry leaves, the greatest care, therefore, being necessitated in the preparation of leaves destined for the food of the worms. But the most noteworthy fact of this Memoir concerns the part in which Béchamp distinguished another silk-worm disease from that of *pébrine*. Observations had already been made by the naturalist, M. N. Joly, upon the presence of vibrios in the intestinal canal of

[1] *Comptes Rendus* 64, p. 835.
[2] *C. R.* 64, p. 1042.
[3] *C. R.*, p. 1043.

sick worms, to which the name of *morts-flats* or *resté-petits* had been given; but as much ignorance prevailed in regard to this disease, which came to be known as *flacherie*, as had existed over *pébrine*.

On the 11th of the previous April, Professor Béchamp had already published a pamphlet on this second silk-worm disease, and afterwards, in July, 1868, forwarded his account to the Academy of Science, which inserted a reference to it.[1] In this pamphlet he wrote:—"A non-corpuscular seed may and often does contain, as observed by M. de Monchy and by me, other products besides the spherules of the vitellus and the fatty globules. They are the motile points, much smaller than all the others that surround them, and often excessively numerous. We call these motile points *microzyma aglaiæ* temporarily, until we determine positively their significance. To sum up, so long as their parents are unknown, the best course will be to procure seed only that is not corpuscular, either internally or externally, and that is free from the *microzyma aglaiæ*."

In his Communication of the 20th May, he went further in his description, and showed that in this other disease the vibrant corpuscles might be entirely absent, while, instead, motile particles were noticeable, like those he had observed in chalk, and equally minute, and on these he now bestowed the name of *microzyma bombycis*, on account of the way in which they were coupled two by two, like a figure of eight.[2]

The next Reports that we find on the subject of silk-worm disease come under the date of the 3rd June, 1867.[3] They are two letters from Pasteur addressed to M. Dumas. Regarding the first the writer has to make a curious explanation. It is dated "Alais, 30th April," and in a note, Pasteur says that this letter left Alais on the 4th May and that by a postal error it only reached Dumas on the 22nd May. Be that as it may, the 30th April is, any way,

[1] *Comptes Rendus* 67, p. 102.

[2] *Les Grands Problèmes Médicaux, par A. Béchamp*, p. 26.

[3] *Comptes Rendus* 64, p. 1109 and *C. R.* 64, p. 1113.

posterior to the 11th April, when Professor Béchamp had put forward his first explanation of *flacherie*; neither does Pasteur in his letter do more than allude to the corpuscular malady as not being the only torment of sericulture. As a safeguard to *pébrine* he put forward his system of taking seed only from moths free from corpuscles, which, as Béchamp pointed out,[1] was an absurdity, considering the parasitic nature of the complaint and the fact that the parasites abounded on mulberry leaves.

The other letter to Dumas, published on the 3rd June, 1867, was dated Alais, the 21st May. Here Pasteur stated that another trouble was often wrongly confounded with *pébrine*, "because in a great number of cases the two diseases had no connection, *or at least not directly*."

Considering the complete disparity of the two complaints, as already shown by Béchamp, the vibrant corpuscles being often entirely absent in the case of *flacherie*, this comment of Pasteur's is noteworthy as showing that he did not possess his rival's comprehension of the subject.

Béchamp, meanwhile, worked hard and sent to the Commission on Sericulture a Memoir entitled: "On the Transformation of the Vibrant Corpuscle of *Pébrine* and on the Nature of the Disease called *Resté-Petits*." This important communication, the Academy of Science published only in abstract on the 10th June, 1867; while on the 1st July of the same year, the Academy published another Memoir, also first sent by Béchamp to the Commission on Sericulture, and entitled: "On the Saccharification of the Vibrant Corpuscle of *Pébrine*." Here he gave a full description of the corpuscle, showing it to lose its oscillating movement in a solution of caustic potash, but to be insoluble in this liquid. He found it to be soluble in sulphuric acid on boiling and proved that glucose could be produced from it by successive treatment with sulphuric acid, barium carbonate, alcohol and water, and came to the conclusion that the vibrant particle contains cellulose.

[1] *Les Grands Problèmes Médicaux*, p. 25.

From Pasteur, the official inquirer into the diseases of silk-worms, the Reports of the Academy of Science provide no further communication on the subject for almost a twelvemonth.

From Béchamp, on the contrary, a series of Memoirs show the way in which his detailed, persevering work on micro-organisms led to his final comprehension of the silk-worm disease called *flacherie*.

He had already, on the 2nd April, 1867, sent up a note to the Academy on "Microscopic Organisms in Saliva." The matter was so new and unexpected that only a *résumé* was given.[1]

On the 24th February, 1868, he sent up a Note on "The Molecular Granulations (microzymas) of Ferments and of Animal Tissues."[2] Here he drew attention to the micro-organisms to be found in vaccine virus, a plagiarised confirmation of which was given by M. Chauveau.

On the 2nd March, 1868, a Note on "The Molecular Granulations (microzymas) of the Cells of the Liver."[3]

On the 4th May, 1868, "On the Origin and Development of Bacteria."[4] This was a general demonstration of bacterial development from the anatomically elemental microzymas.

It was on the 8th June, 1868, that he applied all the preceding facts to the disease of *flacherie* in a Note "On the Microzymian Disease of Silk-Worms."[5] Here he stated *flacherie* to be hereditary owing to the abnormal development of the inherent elemental microzymas of the silk-worm. He showed that the microzymas might be seen singly or associated in chaplets, or in the form of very small bacteria. To see them, a very high power of the microscope was needed, nothing less than obj. 7, oc. I, Nachet. He stated that the microscopes supplied to workers by the

[1] *Comptes Rendus* 64, p. 696.
[2] *C. R.* 66, p. 366.
[3] *C. R.* 66, p. 421.
[4] *C. R.* 66, p. 859.
[5] *C. R.* 66, p. 1160.

Government were not strong enough. He showed that microzymas and bacteria might exist in the same worm, but it appeared worthy of attention that the number of microzymas was in an inverse ratio to that of the bacteria. It was useless to take seed from moths with the complaint, which was distinguishable by an examination of the contents of the abdomen. He pointed out that to isolate the microzymas, they should be treated with a preparation of caustic potash, which, dissolving everything else, would leave the elemental micro-organisms.

Thus, as he had at first fully explained the cause and the mode of prevention of *pébrine*, so now Professor Béchamp made an equally clear and complete explanation of the second silk-worm disease, *flacherie*. He showed that, unlike *pébrine*, it was not caused by an extraneous parasitic invasion, but was due to an abnormal unhealthy development of the microzymas in the body-cells of the silk-worms. The sericultural trouble had given him a chance to demonstrate his full understanding of disease conditions. He was able to provide a clear exposition of, on the one hand, a parasitic complaint, and, on the other, of one due not to a foreign agent, but to a diseased status of anatomical elements.

Pasteur was well acquainted with all the Notes published by Béchamp, but, regrettably to say, had not the generosity to spare praise for his rival's great scientific triumph. It is undeniable that his thought was of himself and how he could best vindicate his own pretensions.

Béchamp's explanation of *flacherie* appeared, as we have shown, among the Reports of the Academy of Science on the 8th June, 1868. On the 29th June, the Reports include[1] a letter to M. Dumas from M. Pasteur dated 24th June, 1868, Paillerols, Commune de Mées, Basses-Alpes. Here it is extraordinary to find that he actually dared to claim that he had been the first to draw attention to this second silk-worm disease and distinguish it from *pébrine*. He wrote to M. Dumas:—"You know that I was the

[1] *Comptes Rendus* 66, p. 1289.

first . . ." But, no doubt, realising that the Academy Reports were destitute of any such proof, he demanded the insertion of the full text of a Note that he claimed to have sent on the 1st June, 1868, to the Agricultural Society of Alais. It was duly inserted with Pasteur's letter, and was entitled: "Note on the Silk-Worm Disease commonly known as *Morts-Blancs* or *Morts-Flats*."

The perusal of these Communications by Pasteur brings home the marvel that he was able to impose upon the world the idea that *he* had elucidated the diseases of silk-worms. Just as he had been astray in regard to *pébrine*, so, even now after all the time he had been at work, he had nothing valuable to impart about *flacherie*. He referred to the organisms associated with the disease, without any allusion to the fact that M. Joly of the Faculty of Science of Toulouse, as well as Professor Béchamp, had observed them long before him. He thought there was nothing to show that these organisms caused the complaint, but that they were the result of digestive trouble. "The intestine," he wrote, "no longer functioning for some unknown reason, the materials it encloses are situated as though inside an immovable vessel."

Béchamp, naturally, felt obliged to answer Pasteur; and so, among the Reports of the French Academy of Science,[1] on the 13th July, 1868, we find a Note from the Professor—"On the Microzymian Disease of Silk-Worms, in Regard to a Recent Communication from M. Pasteur." Here Béchamp refers to his previous pamphlet, published on the 11th April, 1867, in which he and M. Le Ricque de Monchy had drawn attention to the organisms associated with *morts-flats*. He refers to his past Communication of the 13th May, published among the Academy Reports of the 20th May, and also to his Note of the 10th June, 1867. He shows how again on the 28th March, 1868, he published a second edition of his pamphlet, to which he added further opinions on the microzymian complaint, otherwise *flacherie*. He also draws attention to the fact that as

[1] *Comptes Rendus* 67, p. 102.

far back as the 4th July, 1867, a member of the silk-worm industry, M. Raibaud l'Ange, had written to ask to be allowed to visit him at Montpellier to study the disease.

Pasteur responded by calling M. Raibaud l'Ange to his help, only for the latter to confess that he had visited Montpellier for the desired object. Yet, such was the fear of offending the Government representative, the man honoured by Imperial patronage, that M. Raibaud l'Ange, all the same, championed Pasteur with flattery and ridiculed the microzymas.[1]

Béchamp replied to M. Raibaud l'Ange on the 17th August, 1868, reminding him of the table of designs that had accompanied his note of the 8th June, 1867.[2]

No one replied.

As Béchamp afterwards said,[3] the Academy might submit to the plagiarism, but no one could deny it.

No doubt, it was the total inability to set aside Béchamp's just claims that made Pasteur so hate his brilliant rival from this time henceforward. Béchamp's extraordinary success in dealing with the silk-worm diseases was all the more remarkable because he had no help, pecuniary or otherwise, from the Government and no time to expend on the problem except what he could snatch from a professorial career that was filled with work quite apart from any of his scientific researches.

Pasteur, on the other hand, had governmental help at his instant disposal, every expense defrayed and scientific assistants. Moreover, he was given complete leisure to carry out his researches. That another should have so signally succeeded where he had failed must have been a source of bitterness to him, and his jealousy led him into a veritable persecution of Béchamp. He was sure of his own position, which had the highest influence to back it, and we may be certain that he did not allow himself to pass from the memory of his Imperial patrons. He com-

[1] *Comptes Rendus* 67, p. 301.
[2] *C R.* 67, p. 443.
[3] *Les Grands Problèmes Médicaux*, p. 29.

menced his book on vinous fermentation with a foreword to the Emperor, while a dedicatory letter to the Empress in the same way prefaced his book on the disease of silk-worms. We may search in vain through this for any generous reference to the first great elucidator of these troubles. Instead, he takes all the credit to himself[1] and even goes out of his way to deride Béchamp's arguments in favour of creosote as a preventive.[2]

But there is truth in the Yankee dictum that you may fool all the people part of the time and part of the people all of the time, but never all of the people all of the time, and so Pasteur's selfish claims must completely fall to the ground in face of the scientific reports, to which we have given reference, and which are available to anyone, for instance, in the Library of the British Museum. These incontestably prove that the man who made such gains for France in regard to aniline dyes was also the man who provided his country with the correct diagnosis of the silk-worm diseases and suggested methods of prevention.

Unfortunately, practical measures were left to Pasteur, and the best commentary upon these are facts in regard to the sericultural industry put forward by Dr. Lutaud,[3] at one time Editor of the *Journal de Médecine de Paris*.

At the commencement of the silk-worm trouble, about 1850, we are told that France produced annually about 30,000,000 kilogrammes of cocoons. In 1866-67, the production had sunk to 15,000,000 kilogrammes. After the introduction of Pasteur's "preventive method," production diminished from 8,000,000 kilogrammes in 1873 to even so low a figure as 2,000,000 kilogrammes of cocoons in certain subsequent years.

"That is the way," says Dr. Lutaud, "in which Pasteur saved sericulture! The reputation, which he still preserves in this respect among ignoramuses and short-sighted *savants*, has been brought into being, (1) by himself, by

[1] *Études sur la Maladie des Vers-à-Soie, par L. Pasteur* p. 11.

[2] *ibid.* p. 47.

[3] *Études sur la Rage, par le Dr. Lutaud,* pp. 427, 428.

means of inaccurate assertions, (2) by the sellers of microscopic seeds on the Pasteur system, who have realised big benefits at the expense of the cultivators, (3) by the complicity of the Academies and Public Bodies, which, without any investigation, reply to the complaints of the cultivators—'But sericulture is saved! Make use of Pasteur's system!' However, everybody is not disposed to employ a system that consists in enriching oneself by the ruination of others."

Perhaps the greatest harm occasioned by Pasteur's jealousy was the hindrance he set up to notice being taken of Béchamp's work, particularly in regard to his cell doctrine and microzymian theories. So much did Pasteur make it his effort to flout these ideas that, actually, Members of the Academy, influenced by friendly motives, begged Professor Béchamp to drop the very use of the word "microzyma"! Thus the misfortune came about that, instead of being encouraged, science was held back, and at every turn the Professor of Montpellier found himself hampered in the work that, so he believed, would lay the foundations of cytology and physiology and elucidate the processes of the anatomical elements in birth and life, in health and disease, in death and in disruption.

Who gave the Correct Diagnosis of the Silk-Worm Diseases *Pébrine and Flacherie*

BÉCHAMP or PASTEUR?

1865

BÉCHAMP

Statement before the Agricultural Society of Hérault that *Pébrine* is a parasitical complaint and creosote suggested as a preventive of the parasite.

PASTEUR

Statement to the Academy of Science[4] that the corpuscles of *Pébrine* are neither animal nor vegetable. From the point of view of classification should be ranged beside globules of pus, or globules of blood, or better still, granules of starch!

1866

BÉCHAMP

18 June[1]

Statement to the Academy of Science that the disease is parasitical; that *Pébrine* attacks the worms at the start from the outside and that the parasite comes from the air. The disease is not primarily constitutional. Method given for hatching seeds free from *Pébrine*.

13 August[2]

Statement to the Academy of Science describing the parasite as a cell of a vegetable nature.

27 August[3]

Statement to the Academy of Science proving the vibrant corpuscle, *Pébrine*, to be an (organised) ferment.

PASTEUR

23 July[5]

Statement to the Academy of Science that one would be tempted to believe that a parasite had invaded the chambers: *that would be an error*. Inclined to believe that there is no special disease of silk-worms, but that it should be compared to the effects of pulmonary phthisis. Little organisms neither animalcules nor cryptogamic plants.

[1] *Comptes Rendus* 62, p. 1341.
[2] *C. R.* 63, p. 311.
[3] *C. R.* 63, p. 391.

[4] *Comptes Rendus* 61, p. 506.
[5] *C. R.* 63, pp. 126-142.

1867

BÉCHAMP

4 February[1]
Statement to Academy of Science on further research in connection with *Pébrine* as an (organised) ferment.

11 April
Publication of a pamphlet in which attention was called to another silk-worm disease, that of the *morts-flats*, or *resté-petits*, commonly known as *Flacherie.*

29 April[2]
Statement to the Academy of Science on the vibrant corpuscle, *Pébrine*, demonstrating it to be a spore, and supplying a plate of designs. Hope expressed that the priority of his correct diagnosis will not be disputed.

20 May[3]
Statement to the Academy of Science on "New Facts", and the other silk-worm disease, *Flacherie*, clearly distinguished from *Pébrine.*

10 June
Academy of Science published an extract from a Communication on the two diseases previously sent to the Commission on Sericulture.

[1] *Comptes Rendus* 64, p. 231.
[2] *C. R.* 64, p. 873.
[3] *C. R.* 63, p. 1043.

PASTEUR

29 April[4]
Confession of error in having believed, in company with many persons of great repute, that the vibrant corpuscles, *Pébrine*, were analogous to globules of blood, pus, or starch!

3 June[5]
A letter to Dumas communicated to the Academy of Science. Safeguard against disease is to take seed only from moths free from corpuscles (a statement that proves the parasitical nature of *Pébrine* to have been still uncomprehended by Pasteur). An allusion to the corpuscular malady as not the only torment of sericulture.
[6] Another letter to Dumas communicated to the Academy of Science stating another trouble often to be confounded with *Pébrine*, but that "in a great number of cases the two diseases had no connection *or at least not directly!*" (As they had no connection at all, the uncertainty of his ideas is apparent.)

[4] *Comptes Rendus* 64, p. 835.
[5] *C. R.* 64, p. 1109.
[6] *C. R.* 64, p. 1113.

BÉCHAMP

1869

A series of publications, winding up with—

8 June[1]

A communication to the Academy of Science "On the Microzymian Disease of Silk-Worms," more fatal than *Pébrine*, since creosote could be a preventive of the latter, while the former is constitutional and hereditary. The microzymas are to be seen singly or associated in chaplets or in the form of very small bacteria. No seed should be taken from moths that have the complaint discernible by an examination of the contents of the abdomen under a very high power of the microscope, at the very least the combination obj. 7, oc. I, Nachet.

PASTEUR

29 June[2]

A letter to Dumas communicated to the Academy of Science claiming to have been the first to draw attention to the disease of *morts-flats* and demanding the publication of a Communication to the Agricultural Society of Alais on the 1st of the current month.

The latter follows:—Reference to the organisms associated with *Flacherie*, without any acknowledgment of the prior observations of Joly and Béchamp. Considers the organisms to be probably the necessary result of digestive trouble.

COROLLARY

In view of the above, Pasteur's claim of priority in a correct diagnosis of the two silk-worm diseases, repeated on p. 11 of his *Études sur la Maladie des Vers-à-Soie*—IS ENTIRELY WITHOUT FOUNDATION.

[1] *Comptes Rendus* 66, p. 1160.

[2] *Comptes Rendus* 66, p. 1289.

CHAPTER X

Laboratory Experiments

We have already seen that, at the time when Béchamp and Pasteur turned their attention to the subject of fermentation, the vaguest conceptions were held in regard to living matter. Grand names were given, such as protoplasm and *blastème*, but so little was known that the albuminoids were believed to be always identical. Virchow had tried to simplify matters by declaring that the living units of animal and vegetable forms are the cells of the body, while Henle had advanced considerably farther by stating that, on the contrary, the cells are themselves built up by minute atoms, the molecular granulations, just distinguishable within them. Schwann had also taught that the atmosphere is filled with infinitesimal living organisms. Then Béchamp and Pasteur appeared on the scene, the latter, first of all, affirming the spontaneous origin of ferments, while, at the same time, Béchamp irrefutably demonstrated that yeast and other organisms are air-borne. Finally, Pasteur, converted by Béchamp's illuminating views, became enthusiastic over atmospheric germs, and, as we have seen, before a fashionable assembly took to himself the whole credit of their elucidation. Yet so little was he really enlightened that we find him soon afterwards denying the parasitic origin of a complaint, *pébrine*, which was genuinely provoked by a parasite, while, in the opposite direction, his conception of living matter was no further advanced from the old-fashioned view that held the living body to be nothing more than a kind of chemical apparatus. For him in the body there was nothing actually alive; its wonderful workings never suggested to him living autonomous agents.

Of course, in excuse, it may well be said that there was

no reason why Pasteur should have understood the body. He never received any medical, physiological or biological training and had no pretensions to being a naturalist. Chemist though he was, he seems to have had no intuitive sharpness for the branch of science to which he turned his attention. When he took his degree of Bachelor of Science, his examiner appended a note to his diploma stating that he was only "mediocre in chemistry." He does not even seem to have been particularly quick in grasping the ideas of other people, for we have seen what a long time it took before he realised the correctness of Béchamp's explanation of *pébrine.* It was in worldly wisdom that his mind was acute. Fortune favoured him, and he was always on the alert to seize opportunities; but, sad to say, it seems that he was not above pushing himself at someone else's expense, even though the progress of science were thereby hampered, and we can only deplore this misuse of his admirable persistence and energy.

While Pasteur learned nothing more about life than the fact that there are living organisms in the air, Professor Béchamp continued his untiring experiments. Fate was kind in bringing to his help Professor Estor, another worker fully qualified by training and experience. The two scientists were hard-working men, with their minds well exercised by their daily toil, their very discoveries bred, in many cases, by their clinical observations. Béchamp made discoveries in the same way that a Beethoven composes, a Raphael paints and a Dickens writes, that is to say, because he could not help himself, he could not do otherwise. In pathetic contrast, we find men to-day taken away from practical work and set down in laboratories *to make discoveries.* In many cases, they have mediocre minds which could never originate an idea of any sort. All they can follow are routine theories and their so-called "discoveries" are of the type that pile up error upon error. Provide a man with his practical work, and if he have the discoverer's rare insight, as night yields to day, so will practice find enlightenment. What is urgently needed is

freedom from dogma and the encouragement of original opinions. Minds in a mass move at a snail's crawl, and the greatest impediment, no doubt, to Béchamp's microzymian doctrine was the fact that it so utterly outstripped the scientific conceptions of that period.

What he did, first and foremost, was to lay the foundations, of what, even to-day, is a new science, that of cytology.

Having made his surprising discovery of the minute organisms, ágents of fermentation, in chalk, Béchamp's next work was a thorough investigation of the "molecular granulations" of cells with which he connected the "little bodies" of chalk and limestone. Up to this date, Henle's vague views regarding the granulations had been ignored and they were generally considered to be mere formless, meaningless particles. Calling the microscope and polarimeter to his aid and undertaking innumerable chemical experiments, Professor Béchamp, making use at first principally of such organisms as yeast, found the granulations which they contain to be agents provocative of fermentations, and then bestowed on them the explanatory name of microzyma. These same granulations he found in all animal and vegetable cells and tissues and in all organic matter, even though apparently not organised, such as milk, in which he proved them to account for the chemical changes that result in the milk clotting. He found the microzymas teeming everywhere, innumerable in healthy tissues, and in diseased tissues he found them associated with various kinds of bacteria. One axiom he laid down[1] was that though every microzyma is a molecular granulation, not every molecular granulation is a microzyma. Those that are microzymas he found to be powerful in inducing fermentation and to be possessed of some structure. In short, it was made clear to him that they, not the cell, are the primary anatomical elements.

It was never his practice to let his imagination outstrip his experiments. Invariably he propounded his question

[1] *Les Microzymas, par A. Béchamp,* p. 133.

and waited for facts to make answer. Working with Professor Estor, observations showed that not only are the molecular granulations, the microzymas, anatomical elements, autonomously living, with organization and life inseparably united in their minute selves, but that it is due to these myriad lives that cells and tissues are constituted living; in fact that all organisms, whether the one-celled amœba, in its pristine simplicity, or man, in his varied complexity, are associations of these minute living entities.

A modern text-book[1] well sums up Béchamp's primary teaching:—"Their behaviour" (that of the molecular granulations, here named microsomes) "is in some cases such as to have led to the hypothesis long since suggested by Henle (1841) and at a later period developed by Béchamp and Estor and especially by Altmann, that microsomes are actually units or bioblasts, capable of assimilation, growth and division, and hence to be regarded as elementary units of structure, standing between the cell and the ultimate molecules of living matter."

Only some such discovery could clear away the confusion on the subject of spontaneous generation. Superficial observers, among whom we are forced to include Pasteur, continued to maintain that fermentation was only induced by germs from the air; but at the same time Pasteur had to admit that meat, protected from atmospheric contact, in an experiment of his own, none the less, became tainted. Other experimenters insisted upon changes taking place for which atmospheric organisms could not be held responsible.

Béchamp, the first to make clear the fermentative rôle of air-borne agents, was now able, according to his own views, to explain that fermentation might take place apart from these, for all organisms teem with minute living entities capable of producing ferments, and that, in fact, those found in the air he believed to be simply the same released from plant and animal forms, which they have

[1] *The Cell in Development and Inheritance*, by Edmund B. Wilson, Ph.D., p. 290.

first built up, but from which they are afterwards freed by that disruption we call death. The two Professors of Montpellier, working together, began to trace and follow life in its marvellous processes.

At the risk of being wearisome by repetition, we must remind ourselves of the order in which Béchamp achieved his early discoveries. First, he demonstrated that the atmosphere is filled with minute living organisms, capable of causing fermentation in any suitable medium which they chance to light upon, and that the chemical change in the medium is effected by a ferment engendered by them, which ferment may well be compared to the gastric juice of the stomach. Secondly, he found in ordinary chalk, and afterwards in limestone, minute organisms capable of producing fermentative changes, and showed these to bear relation to the infinitesimal granulations he had observed in the cells and tissues of plants and animals. He proved these granulations, which he named microzymas, to have independent individuality and life, and claimed that they are the antecedents of cells, the upbuilders of bodily forms, the real anatomical, incorruptible elements. Thirdly, he set forth that the organisms in the air, the so-called atmospheric germs, are simply either microzymas, or their evolutionary forms set free by disruption from their former vegetable or animal habitat, and that the "little bodies" in the limestone and chalk are the survivors of the living forms of past ages. Fourthly, he claimed that, at this present time, microzymas constantly develop into the low type of living organisms that go by the name of bacteria.

We have already superficially studied the rigid experiments that established Béchamp's views on the fermentative rôle of air-borne organisms and of those found in chalk; let us now follow a very few of the innumerable experiments he carried out in the establishment of his other conclusions. His work was so incessant, his observations so prolific, that only their fringe can be touched and no attempt can be made to trace the exact chronological

order of the experiments upon which he based his opinions.

At a very early stage of his researches. he demonstrated with Professor Estor that air need have nothing to do with the appearance of bacteria in the substance of tissues. Further, these investigators established the independent vitality of the microzymas of certain tissues, certain glands, and so forth, by showing that these minute granules act like organised ferments and that they can develop into bacteria, passing through certain intermediary stages, which they described, and which intermediate stages have been regarded by many authorities *as different species.*

We have seen that the basic solution of the whole secret for Béchamp was his discovery of the "little bodies" in chalk, which possess the power of inverting cane-sugar, liquefying starch, and otherwise proving themselves agents of fermentation. The strata in which he found them were regarded by geologists as having an antiquity of at least eleven million years, and Béchamp questioned whether the "little bodies" he had named *microzyma cretæ* could really be the surviving remains of the fauna and flora of such long-past ages. Not having centuries at his disposal to test the problem, he determined to see for himself what would remain now at this present time of a body buried with strict precautions. He knew that, in the ordinary way, an interred corpse was soon reduced to dust, unless embalmed, or subjected to a very low temperature, in which cases the check to decomposition would be explained by the inherent granules, the microzymas, becoming dormant.

[1]At the beginning of the year 1868, he therefore took the carcass of a kitten and laid it in a bed of pure carbonate of lime, specially prepared and creosoted, while a much thicker layer covered the body. The whole was placed in a glass jar, the open top of which was closed by several sheets of paper placed in such a way that air would be continually renewed without permitting the intrusion of

[1] See *Les Microzymas, par A. Béchamp*, p. 625 and onwards.

dust or organisms. This was left on a shelf in Béchamp's laboratory until the end of the year 1874. The upper bed of carbonate of lime was then removed and proved to be entirely soluble in hydrochloric acid. Some centimeters further down there were only to be found some fragments of bone and dry matter. Not the slightest smell was perceptible, nor was the carbonate of lime discoloured. This artificial chalk was as white as ordinary chalk, and except for a lack of the microscopic crystals of aragonite found in precipitated carbonate of lime, indistinguishable from it, and showed under the microscope brilliant "molecules," such as those seen in the chalk of Sens. One part of this carbonate of lime was then placed in creosoted starch, and another part in creosoted sweetened water. Fermentation took place, just as though ordinary chalk had been used, but more actively. Microzymas were not seen in the upper stratum of the carbonate of lime, but in that portion where the kitten's body had rested they swarmed by thousands in each microscopic field. After filtering the carbonate of lime through a silken sieve, it was taken up with dilute hydrochloric acid, and Béchamp thereby succeeded in separating the microzymas, which had been made visible by the microscope.

At the end of this experiment, which had continued for over six and a half years, Béchamp, with "the infinite patience of genius," repeated it by another, which lasted seven years.

To meet the possible criticism that the body of the kitten had been the prey of germs of the air, which might have been carried in its hair, or admitted into its lungs by breathing, when alive, or into its intestinal canal, Béchamp now repeated his experiment with more rigid precautions.

This time, in addition to burying the whole carcass of a kitten, he also buried, in one case, a kitten's liver, and in another, the heart, lungs and kidneys. These viscera had been plunged into carbolic acid the moment they had been detached from the slaughtered animal. This experiment, commenced in the climate of Montpellier, in the

month of June, 1875, had to be transported to Lille at the end of August, 1876, and was terminated there in August, 1882.

Owing to the temperate climate of Lille, very different from that of Montpellier, which, for a great part of the year, is almost sub-tropical, the destruction of the body was much less advanced in this later experiment than it had been in the previous one. All the same, in the beds of carbonate of lime near the remains, in one case, of the whole kitten, and, in the other, of the viscera, microzymas swarmed and there were also well-formed bacteria. Moreover, the chalk was impregnated with organic matter, which coloured it a yellowish brown, but the whole was odourless.

From these two experiments, Béchamp found great confirmation of views that had been already suggested to him by many other observations. To begin with, they supported his belief that the "little bodies," the microzymas, of natural chalk are the living remains of the plant and animal forms of which in past ages they were the constructive cellular elements. It was shown that, after the death of an organ, its cells disappear, but in their place remain myriads of molecular granulations, otherwise microzymas. Here was remarkable proof of the imperishability of these builders of living forms. Neither is the fact of their own independent life denied by a longevity under conditions that would debar them from nutrition throughout immense periods, since we find prolonged abstention from food to be possible even in the animal world among hibernating creatures, while the naturalist can detail many more cases among minute organisms, for instance, pond-dwellers, which fast for indefinite intervals when deprived of water, their natural habitat, and fern-spores, which also are known to retain a vitality that may lie dormant for many years. Thus, whether confined within some animal or vegetable body, or freed by the disruption of plant and animal forms, the microzymas, according to Béchamp, were proved capable of preserving vitality in a

dormant state, even though the period surpassed men's records. It would still be possible for different microzymas to possess varying degrees of vitality, for, as we shall see, Béchamp found differences between the microzymas of various species and organs.

But, over and above finding that the elements of the cells can live on indefinitely after the disruption of the plant or animal bodies that they originally built up, he considered that he had obtained convincing evidence of their capability of developing into the low types of life known as bacteria. If not, where did these come from in the case of the buried viscera? Even if air-borne germs were not completely excluded in the case of the kitten's body, the utmost precautions had been taken to exclude them in the case of the burial of the inner organs. Yet Béchamp found that the microzymas of the viscera, as well as those of the whole kitten had evolved into associated microzymas, chaplets of microzymas, and finally into fine bacteria, among which the *bacterium capitatum* appeared in the centre of a great piece of flesh.

Here Béchamp saw how wrong, first, the great naturalist, Cuvier, and, after him, Pasteur, had been in assuming "That any part whatever, being separated from the mass of an animal, is by that fact transferred into the order of dead substances and is thereby essentially changed." By Béchamp's researches it was seen that separate parts of a body maintain some degree of independent life, a belief held by certain modern experimenters, who, however, unlike Béchamp, fail to provide an explanation.

His experiment showed the Professor how it is that bacteria may be found in earth where corpses have been buried and also in manured lands and among surroundings of decaying vegetation. According to him, bacteria are not specially created organisms, mysteriously appearing in the atmosphere, but they are the evolutionary forms of microzymas, which build up the cells of plants and animals. After the death of these latter, the bacteria, by their nutritive processes, bring about the disruption, or in

other words, the decomposition of the plant or animal, resulting in a return to forms approximating to microzymas. Thus Béchamp taught that every living being has arisen from the microzyma, and also that "every living being is reducible to the microzyma."[1] This second axiom of his, he says, accounts for the disappearance of bacteria in the earlier experiment, for, just as microzymas may evolve into bacteria, so, according to his teaching, bacteria, by an inverse process, may be reduced to the pristine simplicity of the microzyma. Béchamp believed this to have happened in the earlier case, when the destruction of the kitten's carcass was so much more complete than in the second case, when the temperate climate of Lille had prolonged the process of decomposition.

Many, indeed, were the lessons the indefatigable worker learned from these two series of observations.[2]

1. "That the microzymas are the only non-transitory elements of the organism, which persist after the death of the latter and form bacteria.

2. "That there is produced in the organisms of all living beings, including man, in some part and at a given moment, alcohol, acetic acid and other compounds that are normal products of the activity of organised ferments, and that there is no other natural cause of this production than the normal microzymas of the organism. The presence of alcohol, of acetic acid, etc., in the tissues, reveals one of the causes, independent of the phenomenon of oxidation, of the disappearance of sugar in the organism and of the disappearance of the glucogenic matters and that which Dumas called the respiratory foods.

3. "That, without the concurrence of any outside influence except a suitable temperature, fermentation will go on in a part withdrawn from an animal, such as the egg, milk, liver, muscle, urine, or, in the case of plants, in a germinating seed, or in a fruit which ripens when detached from the tree, etc. The fermentable matter that disappears earliest in an organ after death is the glucose, glucogenic matter or some other of the compounds called carbo-hydrate, that is to say, a respirat-

[1] *Les Microzymas*, p. 925.
[2] *ibid.* pp. 628-630.

ory food. And the new compounds that appear are the same as those produced in the alcoholic, lactic and butyric fermentations of the laboratory; or, during life, alcohol, acetic acid, lactic or sarcolactic acid, etc. . . .

4. "That it is once again proved that the cause of decomposition after death is the same, without the organism, as that which acts, under other conditions, during life, namely, microzymas capable of becoming bacteria by evolution.

5. "That the microzymas, after or before their evolution into bacteria, only attack albuminoid or gelatinous matters after the destruction of the matters called carbohydrates.

6. "That the microzymas and bacteria, having effected the transformations before mentioned, do not die in a closed apparatus in the absence of oxygen; they go into a state of rest, as does the beer-yeast in an environment of the products of the decomposition of the sugar, which products it formed.

7. "It is only under certain conditions, particularly in the presence of oxygen, as in the experiment on the kitten buried in carbonate of lime, etc., that the same microzymas or bacteria effect the definite destruction of vegetable or animal matter, reducing it into carbonic acid, water, nitrogen, or simple nitrogenous compounds, or even into nitric acid, or other nitrates!

8. "That it is in this way that the necessary destruction of the organic matter of an organism is not left to the chances of causes foreign to that organism, and that when everything else has disappeared, bacteria, and, finally, microzymas resulting from their reversion remain as evidence that there was nothing of what was primarily living except themselves in the perished organism. And these microzymas, which appear to us the remains or residuum of that which has lived, still possess some activity of the specific kind that they possessed during the life of the destroyed being. It is thus that the microzymas and bacteria that remained from the corpse of the kitten were not absolutely identical with those of the liver or of the heart, of the lung or of the kidney."

The Professor continued:—"I do not mean to infer that in destruction effected in the open air, on the surface of the ground, other causes do not occur to hasten it. I have never denied that the so-called germs of the air or other causes are contributory. I only say that these germs and these causes have not been expressly created for that purpose and that the so-called germs in atmospheric

dusts are nothing else than the microzymas from organisms destroyed by the mechanism I have just explained and whose destructive influence is added to that of the microzymas belonging to the being in process of destruction. But in the atmospheric dusts there are not only the microzymas; the spores of the entire microscopic flora may intrude, as well as all the moulds that may be born of these spores."

It must not be supposed that Béchamp founded such manifold views upon any mere two series of observations. From the date of his Beacon Experiment, he never ceased from arduous work in connection with micro-organisms. Together with Professor Estor, he instituted many experiments upon inner organs subtracted from fœtuses, accidentally provided for them by abortions. Here again they had overwhelming proof of bacterial evolution from normal inherent particles, for, while they would find bacteria in the interiors, the surrounding liquids, specially prepared as accepted culture media, would be absolutely free from such organisms. They spared themselves no trouble. Space does not allow of more than a trifling reference to a very few of their continual and varied experiments, such, for instance, as those upon eggs, in which, not contenting themselves with hen's, they procured ostrich eggs, with their hard tenacious shells, and subjected these to innumerable tests. From the latter they received evidence of the gradual evolution, in the fecundated egg, of the united microzymas of the male sperm and female germ cells into the organs and tissues of the resultant feathered creature. They were also shown the arrest of this development in eggs that were shaken and disturbed and the internal substitution in the rotting egg of chaplets of associated microzymas and swarming bacteria.

In the course of their work, the Professors applied every possible test to their experiments, sometimes admitting air and sometimes rigorously excluding it. Their observations began to be enthusiastically taken up by some of Professor

Béchamp's pupils, numbered among whom was M. Le Rique de Monchy, who assisted Béchamp with his silkworm researches. In a paper called[1] "Note on the Molecular Granulations of Various Origin," this indefatigable student demonstrated that the vibrating granulations are organisms having an energetic action, similar to that of ferments, upon certain of the matters with which they are in contact in their natural medium.

Meanwhile, his great teacher sent up Memoir after Memoir to the Academy of Science. It was Béchamp who initiated the study of micro-organisms—microzymas and bacteria—in saliva and in the mucus of the nasal and other passages. The very secretions of the body afforded him proof of his opinions. Thus, in a Memoir "On the Nature and Function of the Microzymas of the Liver," he and Estor said:—[2]"Matter, whether albuminoid or other, never spontaneously becomes a zymase or acquires the properties of zymases; wherever these appear some organised (living) thing will be found."

What a wonderful conception this gives of the body! Just as a household or a state cannot prosper without its different members undertaking their varied functions, so our bodies, and those of animals and plants, are regulated by innumerable workers whose failure in action disturbs the equilibrium of the entire organism. Just as in the State there are different experts for different forms of labour, so Béchamp demonstrated the differentiation between the microzymas of various organs, the microzymas of the pancreas, the microzymas of the liver, the kidneys, etc., etc. And since it may be objected that it is too difficult to make such distinctions between microscopic minutiæ, we cannot do better than quote the words of the brilliant experimenter.

"The naturalist," said Béchamp,[3] will not know how to classify them, but the chemist who studies their functions

[1] *Comptes Rendus de l'Académie des Sciences* 66, p. 550.

[2] *Comptes Rendus de l'Académie des Sciences* 66, p. 421 (1868).

[3] *La Théorie du Microzyma*, p. 116.

can do so. Thus a new road is opened: when the microscope becomes powerless to show us among known forms the cause of the transformation of organic matter, the piercing glance of the chemist, armed with the physiological theory of fermentations, will discover behind the chemical phenomena the cause that produces them." Again he said:—"The microzymas can only be distinguished by their function, which may vary even for the same gland and for the same tissue with the age of the animal."[1]

He also showed that they vary for each tissue and for each animal, and that the microzymas found in human blood differ from those found in the blood of animals.

These researches were arousing so much attention that, in 1868, Professor Béchamp was invited by M. Glenard, the Director, to give a special lecture at the School of Medicine at Lyons. On this occasion, the great Master discussed the experiments upon the microzymas of the liver, which he and Professor Estor had conducted together, as well as the rôle that the microscopic organisms of the mouth play in the formation of salivary diastase and in the digestion of starches, which work he had undertaken in connection with Professor Estor and M. Sainte-Pierre. He also pointed out the microzymas in vaccine and in syphilitic pus.

These were the days in which Béchamp was happy in his work at Montpellier, when the star of hope still gleamed, and he displayed the bright cheerfulness habitual to his temperament. We can picture him, with his noble face and large idealistic eyes shining with enthusiasm, as he lectured to his young audience at Lyons. There was never a word of self; of what he had done or hoped to do. Boastings or mock humilities were equally foreign to him. The mysteries of Nature, the workings of life and death, absorbed him. And so the students dispersed with their minds filled with the wonders they had heard and which so far outstripped what they had other-

[1] *Les Grands Problèmes Médicaux, par A. Béchamp*, p. 61.

wise learned that the full meaning, no doubt, barely went home and they had small idea of the genius of the great man, devoid of self-praise, who had lectured so unostentatiously to them.

What wonderful times those were for the great teacher when his views developed with such rapidity, and continuously by day, and often half through the night, he worked at the unravelling of Nature's mysteries; while with him, for a series of years, toiled his devoted colleague, Professor Estor.

"Ah! how moving," wrote Béchamp,[1] "were the innumerable *séances* at which we assisted, amazed by the confirmation of ideas, the verification of facts, and the development of the theory." And with that large-hearted generosity, as natural to him as it was alas! foreign to Pasteur, he added:—"During the period from 1868 to 1876 all that concerns the microzymas of animal organs was common to both of us, and I do not know how to distinguish between what is mine and what is Estor's."

We can faintly realise the emotion of the discoverers as they found themselves penetrating closer to the secrets of life than any man had succeeded in doing before them; exemplifying and proving that which the great Lavoisier had felt after in an earlier epoch. And, since they were both doctors, their labours were not narrowed to the more or less artificial experiments they undertook in the laboratory. Their clinical work brought them constant experience, and their surest observations were those accomplished by the greatest of all experimenters—Nature!

[1] *La Théorie du Microzyma*, p. 123.

CHAPTER XI

Nature's Experiments

We have taken a cursory peep at Béchamp's arduous toil in his laboratory; but he himself would have been the first to insist upon the greater importance he attached to experiments directly undertaken by Nature. To these he gave incessant study. Whenever possible, he would visit the hospital wards and make a close examination of the cases. He carefully followed the medical work of Professor Estor and of the many other doctors with whom he was associated at Montpellier.

A cyst, which required to be excised from a liver, provided a wonderful demonstration of the doctrine of bacterial evolution, for there were found in it microzymas in all stages of development, isolated, associated, elongated; in short, true bacteria. Dr Lionville, one of Béchamp's medical pupils, had his interest greatly aroused, and demonstrated that the contents of a blister include microzymas and that these evolve into bacteria.

With extraordinary patience and industry, Professor Béchamp and his colleagues continued their medical researches, finding the microzymas in all healthy tissues, and microzymas and many forms of bacteria in various phases of development in diseased tissues. Punctuating his clinical study by laboratory tests, the Professor instituted many experiments, which space forbids our enumerating, to prove that the bacterial appearances were not due to external invasions.

[1]One day an accident provided an interesting contribution to the observations. A patient was brought to the hospital of the Medical University of Montpellier suffering from the effects of an excessively violent blow upon the

[1] *Les Microzymas, par A. Béchamp*, p. 181.

elbow. There was a compound comminuted fracture of the articular joints of the forepart of the arm; the elbow was largely open. Amputation was imperative and was performed between seven and eight hours after the accident. Immediately the amputated arm was carried to Dr. Estor's laboratory, where he and Dr. Béchamp examined it. The forearm presented a dry black surface. Complete insensibility had set in before the operation. All the symptoms of gangrene were present. Under a high power of the microscope, microzymas were seen associated and in chaplets, but no actual bacteria. These were merely in process of formation. The changes brought about by the injury had progressed too rapidly to give them time to develop. This evidence against bacteria as the origin of the mortification was so convincing that Professor Estor at once exclaimed: "Bacteria cannot be the cause of gangrene; they are the effects of it."

Here was the outstanding difference between the microzymian theory and its microbian version, which Pasteur and his followers were to be instrumental in promulgating. Pasteur seems to have lacked an understanding of the basic elements of living matter. In life he compared the body to a barrel of beer or a cask of wine.[1] To him, it only appeared an inert collection of chemical compounds; and, therefore, naturally, after death, he recognised nothing living in it. Consequently, when life incontrovertibly appeared, he could only account for it by the invasion from without of those minute air-borne organisms, whose reality Béchamp had taught him to understand. But the explanation of their origin from the cells and tissues of plant and animal forms took him considerably longer to fathom, though, as we shall see, he eventually actually made an unsuccessful attempt to plagiarise Béchamp's point of view.

Béchamp and Estor, meanwhile, steadily persevered with their clinical observations and made a special study, for instance, of microzymian development in cases of

[1] See pp. 79, 80.

pulmonary tuberculosis. The effects they saw in their medical work they proved and tested by laboratory experiments, and with the intense caution of true scientists, they carried out almost innumerable tests to substantiate, for example, their belief in the development of bacteria from microzymas, and the fact that an invasion from without of those at large in the atmosphere is not required to explain their appearance in internal organs.

It was, however, one of Nature's direct experiments, a chance demonstration in the vegetable world, that offered Professor Béchamp one of his best proofs of inner bacterial development, apart from atmospheric interference.

As we have said, the climate of Montpellier is almost sub-tropical for the greater part of the year, and various sun-lovers among plants may be found growing there, including eccentric-looking cacti, with their tough surfaces and formidable prickles. During the winter of 1867 and 1868, however, severe cold set in, and hard frost took liberties with the cacti to which they were quite unaccustomed.[1] On one of these cold winter days, Professor Béchamp's sharp eyes, which never missed anything of importance, noticed an Echinocactus, one of the largest and sturdiest of its kind, frozen for two feet of its massive length. After the thaw set in, the Professor carried off the plant to examine it. In spite of the frost-bite, its surface was so thick and hard that it was absolutely unbroken. The epidermis was as resistant as it had been before the misadventure, and the great density of the tissues safeguarded the interior against any extraneous invasion apart from the intracellular spaces connected with the outer air through the stomata. Yet when the Professor made an incision in the frozen part, he found bacteria teeming inside, the species that he called *bacterium termo* and *putridinis* predominating.

Béchamp at once realised that Nature was carrying out remarkable tests of her workings, and when frost set in again on the 25th January and lasted until the end of the

[1] *Les Microzymas, par A. Béchamp*, p. 141.

same month, he determined to verify his preceding observation. The interesting plants in the Botanical Gardens provided him with fine opportunities, for many of them became frozen.

He started his observations with a cactus named *Opuntia Vulgaris*. This was only frozen in part, and on scraping the surface with a scalpel, the Professor convinced himself that it was entirely unbroken. In his own words, not the minutest cleft had been formed by which an enemy could find access. Yet, all the same, under the skin and down to the deepest layers of the frozen part lurked tiny and very active bacteria and also larger bacteria, equally mobile, of a length of 0.02 mm. to 0.04 mm., though these were less numerous. The normal microzymas had completely given place to bacteria in the frozen parts. On the contrary, it was noteworthy that in the healthy parts, untouched by frost, there were only perfect cells to be found and normal microzymas.

Béchamp next examined a plant known botanically as the *Calla Œthiopica*. This was frozen down to the ground and so perished that the slightest touch made it crumble to powder. Microscopic study showed microzymas in the course of transformation into excessively small mobile bacteria; there were also large bacteria to be seen, measuring 0.03 mm. to 0.05 mm. Nature had also provided a valuable control experiment, for in the centre of the decayed, frozen plant, a bunch of young leaves was left green and healthy, and here only normal microzymas were to be found, in striking contrast to the transformation scenes taking place in the surrounding parts, which the frost had shattered so ruthlessly.

A third illustration was provided by a *Mexican Agave*. In the unfrozen part only normal microzymas were to be found, while in the blackened and frozen portion of the leaf there was a cloud of very mobile microzymas, and there also swarmed bacteria resembling the *bacterium termo*, and in smaller quantities bacteria that measured from 0.01 mm. to 0.03 mm.

In another *Mexican Agave*, the blackened and frozen part of the leaf did not contain any microzymas, but only small bacteria and some longer varieties measuring from 0.008 mm. to 0.02 mm. In the healthy parts, the microzymas were normal, but in proportion as the frozen parts were approached, the microzymas were seen to be modified in shape and size.

A fifth illustration was a *Datura Suaveolens*, in which the ends of the branches were frozen. Under the epidermis, as well as deep below, were clouds of *bacterium termo*, some rare *bacterium volutans* and some large bacteria measuring from 0.03 mm. to 0.04 mm. There were also long crystalline needles terminating in spindles of 0.05 mm. to 0.10 mm., which were motionless and not to be found in the healthy parts. The frozen and withered portions had, all the same, remained green.

Through these and many other observations, Béchamp became convinced that the microzymas of the plant world have great aptitude for developing into bacteria. But as he never jumped to conclusions, he took the utmost care to make perfectly sure that no inoculation of extraneous organisms could in any way be responsible.

A year later, an *Echinocactus Rucarinus*[1] supplied him with an interesting example of the absence of bacteria when their entry from without appeared likely to be facilitated, and thus he seemed to be afforded more proof of his theory that nutritive trouble or a change of environment, like that brought about by frost, may occasion a natural development of internal inherent microzymas.

He happened to enter a conservatory in the Montpellier Botanical Gardens, where he noticed an *Echinocactus*, which in so many ways reminded him of the one he had examined a year before that it seemed as though this one must also have been frost-bitten. He questioned the gardener, who explained that the roots had rotted owing to the plant having been over-watered. Here again was a subject for the persevering student of Nature. We may be

[1] *Les Microzymas*, p. 144.

sure that Professor Béchamp did not miss the opportunity. The hard thick surface seemed to him to be intact, but moulds had been formed by large cells of fungi, which had already developed mycelium. Yet on cutting through this surface, only microzymas and not any bacteria were to be found within the cut, though everything was favourable for an invasion, for there were moulds on the surface and the roots of the plant were rotten.

It is very certain that the Professor, in all the cases we have touched upon, did not content himself with merely a microscopic examination. In each instance, he applied chemical tests and discovered that, roughly speaking, the cell sap of the normal cactus had an acid reaction, whereas that of the frozen parts was found to be slightly alkaline. There were changes, however, which varied with each plant examined, and in a Memoir on the subject,[1] in which these are described, he stated the coincidence of the development of the bacteria and the alkalinity of the medium. He added:—"Although the contrary has been believed, bacteria can develop in an acid medium, which may remain acid or become alkaline, as well as they can develop in an absolutely neutral medium." He believed that if it be true that some species of microzymas evolve into bacteria only in neutral or slightly alkaline media, others, none the less, develop in media normally acid.

Béchamp, as we must remember, had been the first to demonstrate with precision the development and multiplication of air-borne organisms in a suitable medium. Understanding so well the important rôle of the micro-organisms of the air, he was naturally curious to note the effect of their deliberate introduction into surroundings where they would encounter the microzymas, which he considered to be the living formative builders of plant and animal bodies. He, therefore, inoculated plants with bacteria and attentively studied the results of this foreign

[1] *Comptes Rendus de l'Académie des Sciences* 68, p. 466 (22nd February, 1869). *Les Microzymas des Organismes Supérieures, Montpellier Médicale* 24, p. 32. *Les Microzymas*, p. 145.

invasion. In the sugared solutions, which he had used when arriving at the conclusions embodied in his Beacon Experiment of 1857, he had seen the invaders increase and multiply; but now, in the plant interiors, they were in contact with organisms as fully alive as they were. After inoculation, increasing swarms of bacteria were indeed observed, but Béchamp had cause to believe that these were not direct descendants of the invaders. He became convinced that the invasion from without disturbed the inherent microzymas and that the multiplying bacteria he noted in the interior of the plants were, to use his own words,[1] "the abnormal development of constant and normal organisms."

Thus, these experiments, which Nature herself had carried out in the Montpellier Botanical Gardens, were to have far-reaching effects upon Professor Béchamp's pathological teaching. They were to prevent his jumping to hasty conclusions like those, for instance, formulated by Pasteur, who imagined animal and vegetable tissues and fluids to be mere inert chemical media[2] like the sweetened solutions in which Béchamp first displayed the part played by air-borne organisms.

These botanical observations were made by Béchamp at an important epoch when the subject of bacteria was beginning to attract much attention. He made his special study of frost-bitten plants at the commencement of the same year, 1868, in which, later, on the 19th October, Pasteur, at the early age of 45, had the misfortune to be struck down by severe paralysis, brought about, he declared, by "excessive toil" in connection with silk-worm disease. But before this, as we have seen, the celebrated chemist had worked hard to exalt the rôle of what he called the germs of the air and to take to himself the credit of the discovery. His pupils and admirers were content

[1] *Comptes Rendus de l'Académie des Sciences* 66, p. 863.

[2] "*M. Pasteur ne voyait dans un œuf, dans le sang, dans le lait, dans une masse musculaire, que des substances naturelles telles que la vie les élabore et qui ont les vertus de transformation que l'ébullition détruit.*" *Les Microzymas, par A. Béchamp,* p. 15. (*Avant-Propos.*)

to follow his restricted ideas of micro-organisms, and, during the sixties, one of them, M. Davaine, more or less inaugurated what is now known as the germ-theory of disease.

It came about in this way. A complaint called *charbon* or splenic fever, and, later, more commonly known as anthrax, made occasional ravages among the herds of cattle and flocks of sheep in France and other parts of Europe. In 1838, a Frenchman named Delafond drew attention to appearances like little rods in the blood of affected animals, and these were afterwards recognised by Davaine and others. A theory had already been put forward in the past by Kircher, Linné, Raspail and others that special organisms might induce disease, and Davaine, becoming acquainted with Pasteur's idea that each kind of fermentation is produced by a specific germ of the air, now suggested that the little rod-like organisms, which he called bacteridia, might be parasitic invaders of animal bodies and the cause of splenic fever, otherwise anthrax. He and others, who tried to investigate the subject, met with contradictory results in their experiments. It was later, in 1878, that the German doctor, Robert Koch, came to their rescue by cultivating the bacteridia and discovering a formation of spores among them; while Pasteur finally took the matter up and with his fondness for dogmatising declared[1]:—"Anthrax is, therefore, the disease of the bacteridium, as trichinosis is the disease of the trichina, as itch is the disease of its special acarus."

Generalisations are always dangerous in a world of contradictions; but as it has been truly said, "there is no doctrine so false that it does not contain some particle of truth." This wise saying has been quoted by Béchamp,[2] who goes on:—"It is thus with microbian doctrines. Indeed, if in the eyes of a certain number of *savants*, doctors and surgeons, the system of pre-existing morbid germs were denuded of every appearance of truth and did

[1] *The Life of Pasteur*, by René Vallery-Radot, p. 260.
[2] *La Théorie du Microzyma*, p. 37.

not seem established on any experimental reality, its reception by these *savants*, who seem to me to have adopted it without going sufficiently deeply into it, would have been absolutely incomprehensible. Incontestable facts, however, seem to support it. Thus, it is certain that there truly exist microscopic living beings of the most exquisite minuteness, which, undoubtedly, can communicate the specific diseased condition that is in them. The cause both of the virulence and the power of infection in certain products of the sick organism, or of bodies in a state of putrefaction after death, resides in reality in beings of this order. It is true that people have certainly discovered such beings during the development of certain complaints, virulent, infectious, contagious or otherwise."

It is thus seen that it was Béchamp's belief that it is this particle of truth in the germ-theory that has blinded so many to its errors. He explains that the want of a fuller understanding is brought about by lack of sufficient knowledge.

[1]"In my eyes, it is because doctors have perceived no relation, no connecting link, between certain histological elements of the animal and vegetable organism and bacteria that they have so lightly abandoned the laws of the great science to adopt after Davaine, and with Pasteur, Kircher's system of pre-existing disease-germs. Thus it comes about that not understanding the real and essential correlation existing between bacteria and the normal histological elements of our organisation, like Davaine, or denying it, like Pasteur, they have come newly again to believe in the system of P. Kircher. Long before Davaine made his observations and considered the inside of the organism to be a medium for development of inoculated bacteria, Raspail said:—'The organism does not engender disease: it receives it from without. . . . Disease is an effect of which the active cause is external to the organism.' In spite of this, the great physicians affirm, in Pidoux' happy words:—'Disease is born of us and in us.' But M.

[1] *La Théorie du Microzyma*, p. 38.

Pasteur, following the opinion of Raspail, and, trying to verify the hypothesis experimentally, maintains that physicians are in error: the active cause of our maladies resides in disease-germs created at the origin of all things, which, having gained an invisible entry into us, there develop into parasites. For M. Pasteur, as for Raspail, there is no spontaneous disease; without microbes there would be no sicknesses, no matter what we do, despite our imprudences, miseries or vices! The system, neither new nor original, is ingenious, very simple in its subtlety, and, in consequence, easy to understand and to propagate. The most illiterate of human beings to whom one has shown the connection between the acarus and the itch understands that the itch is the disease of the acarus. Thus it comes about that it has seduced many people, who give an unthinking triumph to it. Above all, men of the world are carried away by a specious easy doctrine, all the more applicable to generalities and vague explanations in that it is badly based upon proved and tried scientific demonstrations."

Yes, unfortunately for the great teacher of Montpellier, deeper knowledge, an understanding of that science, cytology, so neglected, as Professor Minchin has complained,[1] even now in the twentieth century, was, and still seems to be, required to comprehend the profounder, more mystic and complicated workings of pathology. Nature was performing experiments, which were open to all to read with the help of the microscope. But few were sufficiently skilled to probe deep enough under what may often be misleading superficialities. Few possessed enough knowledge to understand the complexities revealed to Béchamp. Yet, from the start, he warned the world against being misled by too facile judgments. As early as 1869, he wrote:[2] "In typhoid fever, in gangrene, in anthrax, the existence has been proved of bacteria in the tissues and in the blood, and one was very much disposed

[1] Presidential Address—British Association, September, 1915.
[2] *Comptes Rendus de l'Académie des Sciences* 75, p. 1525.

to take them for granted as cases of ordinary parasitism. It is evident, after what we have said, that instead of maintaining that the affection has had as its origin and cause the introduction into the organism of foreign germs with their consequent action, one should affirm that one only has to do with an alteration of the functions of microzymas, an alteration indicated by the change that has taken place in their form."

The great teacher, who had already so well demonstrated his knowledge of real parasitic disease-conditions by his discovery of the cause of *pébrine*, was surely proving himself to be the best equipped for the understanding of those experiments that Nature undertakes when the normal workings of the body are reduced to chaos, and anarchy reigns in the organism. But the majority of mankind, ignorant of the cytological elements, have been delighted with a crude theory of disease which they could understand and have ignored the profound teaching of Professor Antoine Béchamp. It is to what appears to have been Pasteur's attempted plagiarism of these views that we must now turn our attention.

CHAPTER XII

A Plagiarism Frustrated

A marked contrast between Béchamp and Pasteur lay in the fact that the former demanded a logical sequence between his ideas, while the latter was content to put forward views that were seemingly contradictory one to another. For instance, according to him the body is nothing more than an inert mass, a mere chemical complex, which, while in a state of health, he maintained to be immune against the invasion of foreign organisms.[1] He seems never to have realised that this belief contradicts the germ-theory of disease originally put forward by Kircher and Raspail, which he and Davaine had been so quick in adopting. How can foreign organisms originate disease in a body when, according to Pasteur, they cannot find entry into the self-same body until *after* disease has set in? Anyone with a sense of humour would have noticed an amusing discrepancy in such a contention, but though Pasteur's admirers have acclaimed him as a wit, a sense of the ludicrous is seldom a strong point with anyone who takes himself as seriously as Pasteur did or as seriously as his followers take their admiration of him.

On the 29th June, 1863, he read a Memoir on the subject of putrefaction[2] before the Academy of Science.

In this he said[3]:—"Let a piece of meat be wrapped up completely in a linen cloth soaked in alcohol" (here he copied Béchamp in an earlier experiment) "and placed in a closed receptacle (with or without air matters not) in order to obstruct the evaporation of the alcohol. There will be no putrefaction, neither in the interior, because no

[1] "*Le corps des animaux est fermé, dans les cas ordinaires, à l'introduction des germes des êtres inférieurs.*" *Comptes Rendus de l'Académie des Sciences* 56, p. 1193.
[2] *ibid.* pp. 1189-1194.
[3] *ibid.* p. 1194.

vibrios are there, nor on the outside, because the vapours of the alcohol prevent the development of germs on the surface; but I observed that the meat became *tainted* in a pronounced degree, if small in quantity, and *gangrenous*, if the meat were in considerable mass."

Pasteur's object was to show that there were no inherent living elements in meat, that if external life, the germs of the air, were quite excluded, there would be no bacterial development from inner organisms. These were the days in which, having enthusiastically adopted Béchamp's ideas of the important parts played by the atmospheric hosts, he denied equally vociferously any inherent living elements in animal and vegetable bodies.

Béchamp, knowing how his own skill with the microscope outstripped that of all his contemporaries, excused Pasteur for not having been able to detect the minute organisms in the depth of the fleshy substance. But he maintained that Pasteur's own acknowledgment of the tainted or gangrenous state of the meat should have been sufficient to have convinced him of the reality of a chemical change and its correlative necessity—a causative agent. Béchamp claimed that Pasteur's own experiments, while attempting to deny, on the contrary, proved the truth of the microzymian contentions.

For instance, again, in an experiment on boiled milk, Pasteur observed a smell resembling tallow and noted the separation of the fatty matter in the form of clots. If there were nothing living in the milk, how could he account for the change in its odour and explain the cause of the clotting?

Thus it is impossible to set aside the marked contrast between Béchamp and Pasteur in regard to their attention to any phenomenon, since by the former nothing was ever ignored, while the latter constantly passed over most contradictory evidence. In spite, for example, of all the marked changes in milk, Pasteur was content to describe it as unalterable, except through access of germs of the air, and nothing else than a solution of mineral salts, of milk-sugar and of casein in which were suspended particles of

fat, in short, that it was a mere emulsion which did not contain any living bodies capable of causing any change in its composition. For years Béchamp studied milk, and it was not till a much later date that he finally satisfied himself as to all its scientific complexities.

We find that just as in 1857 Pasteur's sponteparist views were entirely opposed to Béchamp's, so, through the sixties of the nineteenth century, Pasteur completely ignored Béchamp's teaching in regard to the microzymas, or microsomes, of the cells and the fermentative changes due to these inherent living elements. Having realised the germs of the air, he seemed blind to the germs of the body and ignored Béchamp's prodigious work when the latter differentiated by experiment the varying degrees of heat required to destroy the microzymas of milk, chalk, etc. Finally, it seems as though Pasteur must have been convinced against his will by Béchamp's conclusions in regard to the diseases of silk-worms, and his disparagement of the latter was no doubt provoked by his consciousness of a dangerous rivalry. At the end of 1868, laid low on a bed of sickness, who can tell what thoughts passed through his mind in regard to the views of the man who had so enlightened him on the subject of air-borne organisms and their part in fermentation; the man who had so incontestably proved the causes of the diseases of silk-worms that his own scientific reputation had been seriously threatened; the man, in short, who would never be his disciple?

Anyway, when Pasteur rose from his sick bed, semi-paralysed, dragging one leg, the Prussian hordes for a time interrupted the even tenor of French life and national distress annihilated minor controversies. Who shall say if he thought these catastrophic events likely to have a lethal effect on the memories of his contemporaries? Be that as it may, in the year 1872, Pasteur suddenly sprang a surprise upon the Academy of Science.

For a moment, we must recapitulate. It will be remembered that as early as 1862, Béchamp took up the study of

vinous fermentation and the results of his experiments were published in 1864, when he stated clearly that from the outside of the grape comes the mould that causes must to ferment and that the stalks and leaves of vines bear organisms that may produce a fermentation injurious to the vintage. He showed here his extensive view of fermentative phenomena. Not only did he understand the part played by air-borne organisms and the rôle of indwelling cellular elements, but he was also able to point to organisms found on external surfaces. Subsequently, from the year 1869 to 1872, two other experimenters, Lechartier and Bellamy, bore out his views by demonstrating that the intracellular elements of fruits ferment and furnish alcohol when protected from air, the fermentation being in relation to the vegetative activity.

While this solid work was quietly progressing, Pasteur, on his part, was gaining great public attention. We have seen, how, at the start, he was fortified with the Emperor's blessing, and he dedicated to Napoleon III the book for which he was given the grand prize medal of the exhibition of 1867. Indeed, to receive it, he made a special pilgrimage to Paris, where, as his biographer naively suggests,[1] "his presence was not absolutely necessary." One would have imagined that, after so much worldly success, he would have been ready to give credit where credit was due in regard to views diametrically opposed to his incessant invocation of atmospheric germs in sole explanation of fermentative phenomena. But we fear that even his admirers must admit that to give place to others was scarcely a habit of Pasteur's; that is, not unless the others acknowledged him to be the sun, when he, in return, was ready to shed lustre on them as his satellites. Had Béchamp first bowed the knee to him, he might have been ready to accord a meed of praise to the Professor; but as the latter outstripped and criticised him, the two were always at variance, even on points where their views might have been assimilated.

[1] *Life of Pasteur*, by René Vallery-Radot, p. 141.

Pasteur, as we have already said, sprang a surprise upon the Academy in 1872, a year memorable for the incessant work undertaken by the School of Montpellier.

To take merely the end of the year, we find on the 7th October, 1872, an extract read before the Academy from a Note of Béchamp's, entitled, "Upon the Action of Borax in the Phenomena of Fermentation."[1] This was of considerable interest at that time and answered certain questions raised by M. Dumas.

On the 21st October, 1872, Professor Béchamp and Professor Estor presented a joint Memoir "On the Function of the microzymas during Embryonic Development."[2] This was one of the many highly important treatises upon striking discoveries and the experiments that substantiated them.

On the 28th October, 1872, Béchamp read a Memoir entitled "Researches upon the Physiological Theory of Alcoholic Fermentation by Beer-Yeast."[3]

On the 11th November of the same year, he read a Memoir on "Researches upon the Function and Transformation of Moulds."[4]

Some idea of his incessant toil may be gleaned from the mere titles of these records of his untiring energy. We can, therefore, picture his astonishment and natural chagrin when he was roused from his arduous researches by Pasteur's appropriation of views that he had put forward years previously.

First of all, on the 7th October, 1872, Pasteur described to the Academy "Some New Experiments Showing that the Yeast-Germ that Produces Wine Comes from Outside the Grape."[5]

Here was Béchamp's discovery, published in 1854!

This was too much even for the subservient Members of the Academy! M. Fremy interrupted, with the object of exposing the insufficiency of Pasteur's conclusions.

[1] *Comptes Rendus de l'Académie des Sciences* 75, pp. 837-839.

[2] *C. R.* 75, pp. 962-966.

[3] *C. R.* 75, pp. 1036-1040.

[4] *C. R.* 75, p. 1199.

[5] *C. R.* 75, p. 781.

On the invitation of M. Dumas, Pasteur renewed his Address to the Academy, under the title of "New Facts to Assist to a Knowledge of the Theory of Fermentations, properly so-called."[1]

Here Pasteur made the statement in which he claimed "to separate the chemical phenomena of fermentations from a crowd of others and particularly from the acts of ordinary life," in which, of course, nutrition and digestion must be paramount. Here we clearly see that as late as 1872, while theorising upon fermentation, he had no real conception of the process, no clear understanding of it as a function of nourishment and elimination on the part of living organisms. How little foundation is shown for the statement made later by his disciple, M. Roux, "The medical work of Pasteur commences with the study of fermentation."

Proceeding with his address, Pasteur claimed to have shown that fermentation is a necessary consequence of the manifestation of life when that life is accomplished outside of direct combustion due to free oxygen. Then he continued:—"One perceives as a consequence of this theory that every being, every organ, every cell that lives or continues its life without the help of the oxygen of the air, or uses it in an insufficient degree for the whole of the phenomena of its proper nutrition, must possess the character of a ferment for the matter that serves as a source of heat, wholly or in part. This matter seems necessarily to contain carbon and oxygen, since, as I have shown, it serves as food to the ferment. . . . I now bring to this new theory, which I have already several times proposed, although timidly, since the year 1861, the support of new facts, which I hope will this time compel conviction." After a description of experiments mere copies of those undertaken by others, he wound up triumphantly:—"I already foresee by the results of my efforts that a new path will be opened to physiology and medical pathology."

[1] *Comptes Rendus* 75, p. 784.

The only timidity apparent is the wariness with which Pasteur put forward a conviction that "every being, every organ, every cell must possess the character of a ferment." Such teaching was entirely opposed to the theories he had formulated since 1861 and really seems to have been nothing less than a cautious attempt to plagiarise Béchamp's microzymian doctrine. As we have seen, Béchamp, though maintaining that the grape, like other living things, contains within itself minute organisms, microzymas, capable of producing fermentation, yet ascribed that particular fermentation known as vinous to a more powerful force than these, namely, organisms found on the surface of the grape, possibly originally air-borne. Therefore, if Pasteur were accused of plagiarising Béchamp's microzymian ideas, he had only to deny the accusation by pointing out that the provocative cause of vinous fermentation came from outside the grape; though, here again, he was only following Béchamp. The Reports of the Academy of Science show us how well the clever diplomatist made use of these safeguards.

M. Fremy was quick to return to the contest. In a Note upon the Generation of Ferments,[1] he said:—"I find in this Communication of M. Pasteur a fact that seems to me a striking confirmation of the theory that I maintain and which entirely overturns that of my learned *confrère*. M. Pasteur, wishing to show that certain organisms, such as the alcoholic ferment, can develop and live without oxygen, asserts that the grape, placed in pure carbonic acid, can, after a certain time, ferment and produce alcohol and carbonic acid. How can this observation agree with the theory of M. Pasteur according to which ferments are produced only by germs existing in the air? Is it not clear that if a fruit ferments in carbonic acid, consequently under conditions in which it can receive nothing from the air, it must be that the ferments are produced directly under the influence of the organisation within the interior of the cells themselves and that their generation is

[1] *Comptes Rendus* 75, p. 790.

not due to germs that exist in the air. More than ever, then, I reject this theory of M. Pasteur that derives all fermentations from germs of ferments, which, though never demonstrated, are yet said by him to exist in the air; and I maintain that the phenomena due to atmospheric spores must not be confused with those produced by the actual ferments begotten by the organisation."

M. Pasteur replied:—"M. Fremy seems not to have understood me. I have carefully studied the interior of fruits used in experiments, and I assert that there were not developed either cells of yeast or *any organised ferment whatever*."

The argument between the two continued and grew heated; till Pasteur, losing his temper, accused M. Fremy of making himself the champion of German science; though, at the same time, he expressed regret at overstepping the bounds of courtesy.

After some more argument, M. Fremy accepted Pasteur's apology; though he hoped he would not repeat such an offensive observation as that about the Germans; for then, as again afterwards at the time of the Great War, there was naturally such a prejudice against everything Teutonic that not even German science could be excepted.

M. Fremy then went on further to criticise Pasteur's contentions[1]:—"Our *confrère* imagines that he will issue victorious from the discussion that I sustain against him, if the exactness of the facts that he presents be not contested. M. Pasteur deceives himself strangely as to the actual basis of the discussion. It relates not only to the determination of certain experimental facts, but also to their interpretation."

Pasteur, tentatively trying to put forward Béchamp's microzymian views, was now faced by M. Fremy with his actual theories of the past decade. M. Fremy tried to entangle him in them and at the same time expose the shallowness of the theory of air-borne germs as the explanation of *all* vital phenomena. To defend it, Pasteur

[1] *Comptes Rendus* 75, pp. 1059, 1060.

was obliged, so M. Fremy pointed out, to account for each kind of fermentation as the work of a special organism. Then again, if fermentations were only produced by atmospheric germs, they could not take place when air had been purified by rain, or on mountain heights, which Pasteur himself had described as free from such organisms. And yet it was indisputable that fermentations are produced everywhere, even after rain, and upon the highest mountains.

"If the air," said M. Fremy, "contained, as asserted by M. Pasteur, all the germs of ferments, a sweetened liquid, capable of developing ferments, should ferment and present all the successive changes experienced by milk or barley-meal—a thing that never happens."

M. Fremy persisted that it was established that organised bodies, like moulds, elaborate ferments; and that though Pasteur had always declared fermentation to result from the action of atmospheric corpuscles, he, M. Fremy, had long since demonstrated that when the seeds of barley are left in sweetened water, a fermentation is produced in the interior—an intracellular fermentation, carbon dioxide being eliminated from the cells. Fremy claimed that this intracellular fermentation gave the final blow to Pasteur's theory and he derided Pasteur for declaring the production of alcohol within the cells not to be fermentation because of the absence in the fruit juices of specific beer-yeast. He pointed out that actual ferments are secreted inside organisms, instancing pepsin, secreted by the digestive apparatus, and diastase, produced during the germination of barley. He showed that in these cases the ferments themselves are not visible, but only the organs that secrete them; and that though known ferments, such as yeast, are not found in intracellular fermentations, that is no proof that fermentation does not occur.

He contended that "a fermentation is defined not by the ferment that causes it, but by the products that characterise it. I give the name of alcoholic fermentation to every organic modification that in decomposing sugar produces

chiefly carbon dioxide and alcohol. The lactic fermentation is characterised by the transformation of sugar or dextrin into lactic acid. The diastasic ferment is that which changes starch first into dextrin and then into glucose. It is thus that, in my idea, fermentation must be defined. If, as desired by M. Pasteur, one rests the definition of ferments upon the description of the forms that the ferments may take, serious errors are likely to arise."

Finally, he wound up:—"In conclusion, I wish to refute a sort of accusation often reproduced in the communications of M. Pasteur. Our *confrère* accuses me of being almost alone in maintaining the opinions I have above developed. I do not know that M. Pasteur is justified in saying that all *savants* share his opinions upon the generation and mode of action of ferments. I know a certain number of *savants* of full competence in these matters, Members of the Academy and others, who do not agree with M. Pasteur."

In the course of the controversy M. Fremy distinctly showed that he did not rest his opposition to M. Pasteur on the accuracy or inaccuracy of his experiments, but upon the conclusions drawn from them, which he considered to be incorrect. Pasteur, artfully, refused to consider the subject from this point of view and called for a Commission of Members of the Academy to judge of the accuracy of his experiments without regard to his interpretation of results! M. Fremy pointed out that to do this would be to beg the real question at issue,[1] and the matter ended in the two men continuing to slap at each other, Pasteur trying to make capital out of the fact that Fremy saw no use in the suggested Commission.

Pasteur also fell foul of the botanist, M. Trecul, in regard to a Note that had not been read aloud at the Session of the Academy on November 11th.[2] At the Session held on the 18th November, Trecul expressed regret that Pasteur

[1] *Comptes Rendus* 75, pp. 1063-1065.
[2] *C. R.* 75, p. 1168.

had seen fit to add this Note, which is of considerable importance, being tantamount to a complete confession that about four months previously he began to have doubts in regard to the transformation of the cells of the organism he called *mycodermi vini* into yeast cells, and now was prepared to deny M. Trecul's belief in a transformation of cells.

He condescendingly warned him:—"Let M. Trecul appreciate the difficulty of rigorous conclusions in these delicate studies."

To which M. Trecul retorted[1]:—"There is no need to caution me as to the causes of error that may present themselves in the course of such experiments. I pointed them out in 1868 and in 1871 in four different Communications and have since written lengthily upon them." He added:—"M. Pasteur said in the Communication of the 7th October and in his reply to M. Fremy of the 28th of the same month, 1st, that the cells of grapes and of other fruits placed in carbonic acid immediately form alcohol; 2nd, that there is no appearance of yeast in their interior; 3rd, that it is only in rare and exceptional cases that cells of yeast can penetrate from the outside to the inside."

M. Trecul found these statements confusing in view of another made by Pasteur[2]:—"In the gooseberry, fruit of quite another nature to grapes and apples, it often happened to me to observe the presence of the small yeast of acid fruits."

"How," asked M. Trecul, "can this penetration of the beer-yeast take place into the interior of fruits that have intact surfaces?"

It is not altogether surprising that such contrary statements on this and other subjects should have driven Trecul to complain of Pasteur's mode of argument,[3] which he said consisted of contradicting himself, altering the sense of words, and then accusing his opponent of the alteration.

[1] *Comptes Rendus* 75, p. 1219.
[2] *C. R.* 75, p. 983.
[3] *C. R.* 88, p. 249.

Trecul himself experienced "many examples of the contradictions of our *confrère*, who has nearly always two opposite opinions on every question, which he invokes according to circumstances."[1]

But while many realised that Pasteur could not support his new without giving the lie to his old theories, none could understand as clearly as the workers of Montpellier his tentative effort to capture Béchamp's teaching and put it forward, dressed in new words, as his own scientific offspring. This was too much for the Professor's patience, and on the 18th November, 1872, we find a Note presented by him to the Academy on[2] "Observations Relating to some Communications recently made by M. Pasteur and especially upon the Subject 'The Yeast that Makes the Wine Comes from the Exterior of the Grape.' "

In this Memoir, Béchamp referred to his early experiments on vinous fermentation, which had been published in 1864. He added:—"M. Pasteur has discovered what was already known; he has simply confirmed my work; in 1872 he has reached the conclusion arrived at by me eight years before, namely: that the ferment that causes the must to ferment is a mould that comes from the outside of the grape; I went further; in 1864, I established that the stalks of the grape and the leaves of the vine bear ferments capable of causing both sugar and must to ferment, and further that the ferments borne on the leaves and stalks are sometimes of a kind to injure the vintage."

Béchamp now also took the opportunity of bringing before the Academy the conclusions of a note presented by him previously on the 15th February, 1872. This had been omitted, ostensibly on account of its length, but the need for its publication was now apparent, and its previous omission illustrates in a small degree the annoyance to which he was continually subjected. But it was not until the Session of the Academy on the 2nd December, 1872, that the Professor dealt with the deeper significance of

[1] *Le Transformism Médical, par M. Grasset*, p. 136.
[2] *Comptes Rendus* 75, pp. 1284-1287.

Pasteur's newly expressed views. In his Memoir entitled[1] "Second Observation on some Recent Communications by M. Pasteur, notably on the Theory of Alcoholic Fermentation," Béchamp commenced with a restrained and dignified protest:—

"Under the title 'New Facts to Forward the Knowledge of the Theory of Fermentations, Properly So-called,' M. Pasteur has published a Note, the perusal of which has interested me all the more in that I have found many ideas in it that have been familiar to me for a long time. My deep respect for the Academy and consideration for my own dignity impose upon me the obligation of presenting some observations on this communication, otherwise, people who are not in touch with the question might believe that I had imposed on the public by attributing to myself facts and ideas that are not mine."

He went on to show by dates and by quotations from numerous works that he had been the first to establish two essential points: 1st. That organised and living ferments could be generated in media deprived of albuminoid matter. 2nd. That the phenomena of fermentation by organised or "figured" ferments are essentially acts of nutrition.

One single fact surely deals the death-stroke to the claim that Pasteur initiated a true understanding of fermentation, and that is that in his earlier experiments, those of 1857, for instance, and again in 1860, he employed proteid matters and thus showed that he had missed the whole point of Béchamp's great discovery that organised living ferments could arise in media totally devoid of anything albuminoid. The life at large in the atmosphere could only be demonstrated by its invasion of a purely chemical medium entirely free from the suspicion of any organised living elements. This solitary fact gives evidence that Pasteur did not then understand the real significance of Béchamp's demonstration.

The latter now went on to describe the physiological

[1] *Comptes Rendus* 75, p. 1519.

theory of fermentation as proved by his past experiments:—"For me alcoholic and other fermentations by organised ferments are not fermentations in the proper sense of the term; they are acts of nutrition, that is to say, of digestion, of assimilation and of excretion.

"Yeast transforms first of all, outside of itself, cane-sugar into glucose by means of a substance that it contains fully formed in its organism, and which I have named zymase: it then absorbs this glucose and nourishes itself on it: it assimilates, multiplies, increases and excretes. It assimilates, that is to say, a portion of the modified fermentible matter becomes momentarily or definitely a part of its being and serves towards its growth and its life. It excretes, that is to say, it expels the parts used by its tissues under the form of compounds that are the products of fermentation.

"M. Pasteur objected that acetic acid, the constant formation of which I had demonstrated in alcoholic fermentation, had its source not in the sugar, but in the yeast. To this question on the origin of the products of fermentation, which so greatly occupied M. Pasteur and his disciples, I made answer:—They ought, according to the theory, to come from the yeast in the same way that urea comes from us, that is to say, from the materials that at first composed our organism. In the same way that the sugar, which M. Claude Bernard saw being formed in the liver, comes from the liver and not directly from food, so alcohol comes from yeast. This is what I call the physiological theory of fermentation. Since 1864, all my efforts have been directed to the development of this theory: I developed it at a Conference held at Montpellier and at another held at Lyons. The more I insisted on it, the more it was attacked. Attacked by whom? We shall see."

Béchamp then went on to show that it had been M. Pasteur and his pupil, M. Duclaux, who had been the chief opponents of this teaching. He quoted M. Duclaux as having said :—"M. Béchamp has not observed that

there might be two quite distinct sources from which they (the volatile acids of fermentation) might proceed, namely, the sugar and the yeast." He also again quoted M. Duclaux's extraordinary misconception of digestion as exposed by his statement: "When one sees in an alcoholic fermentation a given weight of sugar transformed into alcohol by a weight of yeast a hundred or a thousand times smaller, it is very difficult to believe that this sugar ever made part of the material of the yeast, and that it (the alcohol) is something like a product of excretion."

This misconception Béchamp showed to be now echoed by M. Pasteur in the Memoir under discussion, in which the latter stated:—"That which separates the chemical phenomena of fermentation from a crowd of others, and particularly from the acts of ordinary life, is the fact of the decomposition of a weight of fermentative matter greater than the weight of the ferment in action."

The Professor repeated the explanation he had given in 1867 in answer to such crude objections. He had then shown that they could only have been made by those ignorant of physiological processes and had put forward the simile of a centenarian, weighing 60 kilogrammes, who, in addition to other food, could have consumed something like the equivalent of 20,000 kilogrammes of meat and have produced, more or less, 800 kilogrammes of urea. "Thus," Béchamp concluded, "it is impossible to admit that M. Pasteur has founded the physiological theory of fermentation regarded as a phenomenon of nutrition. That *savant* and his disciples have taken the opposite view. I ask the Academy to permit me to record this conversion of M. Pasteur."

So far, Professor Béchamp had ignored Pasteur's final attempt at plagiarism; but now, at the same Session of the Academy, on December 2nd, together with Professor Estor, he presented a joint Note, entitled: "Observations upon the Communication made by M. Pasteur the 7th October, 1872."[1]

[1] *Comptes Rendus* 75, p. 1523.

Nothing can surpass the dignity with which the two great workers dealt with the subject.

"M. Pasteur," they said, "at the Academy on the 7th October last, announced new experiments on the rôle of cells in general, considered as agents of fermentation in certain circumstances. The principal conclusions of his Communication are as follows:—

"1. All beings are ferments in certain conditions of their life, for there are none in which the action of free oxygen may not be momentarily suspended.

"2. The cell does not die at the same time as the being or organ of which it forms a part.

"3. M. Pasteur foresees, from results already obtained, that a new path is opened to medical physiology and pathology."

Béchamp and Estor showed that, for a long time past, it was they who had taught that every being, or rather every organ in such a being and every collection of cells in such an organ, could play the part of ferments, and it was they who had shown the minute cellular particles that are the agents of fermentative activity. It was Béchamp who had demonstrated that the egg "contains nothing organised except microzymas; everything in the egg, from the chemical point of view, will be necessary for the work of the microzymas; if in this egg its ordered procedure should be disturbed by a violent shaking, what happens? The albuminoid substances and the bodies of fat remain unchanged, the sugar and the glucogen disappear, and in their place, are found alcohol, acetic acid and butyric acid; a perfectly characterised fermentation has taken place there. That is the work of the microzymas, the minute ferments, which are the agents and the cause of all the observed phenomena. And when the bird's egg has accomplished its function, which is to produce a bird, have the microzymas disappeared? No; they may be traced in all the histological elements; they pre-exist, one finds them again during the functioning and the life of the elements; one will find them yet again after death; it

is by them that the tissues are made alive. The part of organised beings essentially active and living, according to the physiologists, is the granular protoplasm. We went a step further and said it is the granulations of the protoplasm, and though for their perception a sort of spiritual insight is required, we have based our conclusions upon experimental proofs of the most varied and positive nature. Bichat looked upon the tissues as the elements of the bodies of the higher animals. With the help of the microscope, very definite particles, cells, were discovered, and were regarded in their turn as elementary parts, as the last term of the analysis, as a sort of living molecule. We have said in our turn:—The cell is an aggregate of a number of minute beings, having an independent life, a separate natural history. Of this natural history we have made a complete description. We have seen the microzymas of animal cells associate two by two, or in larger numbers, and lengthen into bacteria. . . . We have studied the function of these microphytic ferments in physiology, in pathology and after death. We have first determined their importance in the functioning of secretions and shown that this functioning is, after all, only a special mode of nutrition. We have considered them as builders of cells. . . . We have also announced the importance of microzymas in pathology: 'In typhoid fever,' we said in 1869, 'in gangrene, in anthrax, the presence of bacteria has been established in the tissues and in the blood, and there has been a strong disposition to look upon this as a fact of ordinary parasitism. It is evident, after what we have said, that instead of maintaining that the disorder has for source and cause the introduction into the organism of foreign germs with their consequent action, it should instead be affirmed that it is only a matter of a deviation from the normal functioning of microzymas, indicated by the change effected in their form.' (*Congrès Médical de Montpellier, 1869. Montpellier Médical, Janvier, 1870.*) . . . All modern works on contagion and viruses are baseless outside the doctrine of the

microzymas. After death, we said again, at the Medical Congress of Montpellier, in 1869, it is necessary for matter to return to its primitive state, for it has only been lent for a time to the organised living being. In these latter days an excessive rôle has been ascribed to germs carried by the air; the air may bring them, true enough, but they are not essential. The microzymas, in their bacterial stage, are sufficient to assure, by putrefaction, the circulation of matter. We have thus demonstrated for a long time, not only that cells can behave as ferments, but also which are the parts in them that undertake this rôle. The cell, it is said, does not die at the same time as the being or the organ of which it forms a part. This proposition is badly expressed. The cell dies fast enough, if one considers as such the external envelope or even the nucleus. It is known that it is impossible to study histology on a corpse, so capable is it of varied fermentations; a few hours after death, it is sometimes impossible to find a single epithelial cell intact. What should be stated is that the whole cell does not die; this we have demonstrated for a long time by rearing the parts in them that survive. M. Pasteur foresees that a new path will be opened in physiology. In 1869 we wrote as an epitome of all our preceding work:—'The living being, teeming with microzymas, carries in himself with these microphytic ferments the essential elements of life, of disease, of death and of complete destruction.' This new path, we have not only foreseen, but have actually opened many years ago and have persistently pursued it."

In face of this restrained but damning protest, Pasteur could not keep silent. So we find that, on the 9th December, he presented to the Academy "Observations on the Subject of Three Notes Communicated at the Last Session by Messrs. Béchamp and Estor."[1]

"I have read with attention," he said, "these Notes or claims of priority. I find in them only appreciations, the truth of which I believe I am authorized to dispute, and

[1] *Comptes Rendus* 75, p. 1573.

some theories, the responsibility for which I leave to their authors. Later, and at my leisure, I will justify this judgment."

But, apparently, the leisure was never accorded him. Pasteur relapsed into silence.

No "justification of his judgment" being forthcoming, Professor Béchamp and Professor Estor sent up the following Note on the 30th December, 1872.[1] "We beg the Academy to permit us to place on record that the observations inserted in the name of M. Béchamp and of ourselves, on pages 1284, 1519, and 1523 of the present volume of the *Comptes Rendus*, remain unanswered."

The facts indeed seem unanswerable. The famous chemist who had gained the ear of the Public, that exceedingly credulous organ, and had put forward as his own so much of Béchamp's teaching, was now completely checked in his attempted incursion into the microzymian doctrine. Here he had to cry a halt and content himself with his own assertion that "fermentation is life without air, without oxygen." To this, applying his own approved test of time, we find his admirers regretfully acknowledging the deficiencies of his explanation.

"It would be out of place here," say his biographers, Professor and Mrs. Frankland,[2] "to discuss the criticisms which at the present day are being actively carried on; one of the principal objections to the acceptance of Pasteur's views being the omission of all consideration of the element of time in estimating the fermentative power of yeast. . . . Within the present year (1897) the discovery has been made by E. Büchner that a soluble principle giving rise to the alcoholic fermentation of sugar may be extracted from yeast cells, and for which the name of *zymase* is proposed. This important discovery should throw a new light on the theory of fermentation, as it will soon be possible to attack the problem in a new and much more decisive manner. Thus it is presumably very

[1] *Comptes Rendus* 75, p. 1831.
[2] *Pasteur*, by Professor and Mrs. Frankland, chap. IX.

improbable that the action of this soluble zymase is influenced by the presence or absence of air. . . ."

Thus the test of time makes answer to the pronouncements of Pasteur! And if his exponents would only study the old records of the French Academy of Science, as well as the panegyrics of a dutiful son-in-law,[1] not only might their point of view undergo a change, but they would be spared the blunder of attributing to Büchner at the end of the nineteenth century a discovery made by Professor Antoine Béchamp little more than mid-way through that "Wonderful Century"!

[1] M. René Vallery-Radot.

Who First Discovered the Cause of

Vinous Fermentation—

BÉCHAMP or PASTEUR?

BÉCHAMP	PASTEUR
1864	1872
10 October	7 October
Communication to the Academy of Science[2] on "The Origin of Vinous Fermentation." An account of experiments that prove vinous fermentation to be due to organisms on the skin of grapes and also found on the leaves and other parts of the vine, so that diseased vines may affect the quality of the fermentation and the wines that result from it.	Communication to the Academy of Science[3] on "New Experiments to Demonstrate that the yeast-germ that makes wine comes from the exterior of grapes."

Corollary

That Béchamp's discovery antedated Pasteur's by eight years and that his explanation was considerably fuller.

[2] *Comptes Rendus* 59, p. 626.

[3] *Comptes Rendus* 74, p. 781.

Did Pasteur come to acknowledge Béchamp's contention that there is fermentation apart from the action of air-borne organisms, but fail to substantiate any claim to this discovery?

1872

BÉCHAMP & ESTOR

2 December

Communication to the Academy of Science[1] on "Observations upon M. Pasteur's Note of the 7th October." It was shown that it was they who for many years past had taught that every being, or rather every organ in such a being and every collection of cells in such an organ, could play the part of ferments by means of the minute cellular particles, the fermentative agents.

The new path to physiology they had not only foreseen, but had opened up and persistently pursued for many years.

30 December

A Note to the Academy of Science[2] asking for the fact to be recorded that their observations on M. Pasteur's Communication remain unanswered.

[1] *Comptes Rendus* 75, p. 1523.
[2] *C. R.* 75, p. 1831.

PASTEUR

7 October

Communication to the Academy of Science[3] that "Every being, every organ, every cell that lives without the help of oxygen must possess the character of a ferment."

The opening foreseen of "a new path to physiology and medical pathology."

9 December

Expressed to the Academy of Science,[4] hope to be able later, at his leisure, to dispute the Communication of Messrs. Béchamp and Estor.

[3] *Comptes Rendus* 75, p. 785.
[4] *C. R.* 75, p. 1573.

CHAPTER XIII

Microzymas in General

So much worldly success had fallen to the lot of Pasteur that he was little accustomed to checks from his contemporaries. There seems small doubt that his rancour against Béchamp was considerably increased by the latter's determination to safeguard himself against any plagiarism of his theories concerning the cell and its formative elements. If the microzymian doctrine, suitably disguised, could not be put forward as Pasteur's, so much the worse for the microzymas and all that concerned them. The standing that the renowned chemist had achieved made it easy for him to trample upon any scientific growth likely to overshadow his own achievements, and, with his extraordinary good luck, circumstances again abetted him.

The time had come when Professor Béchamp relinquished his important post at Montpellier in the hope of benefiting his country. His gifted young son, Joseph, who was proving a worthy helper in his researches, followed his example. The whole family, with the exception of the elder daughter, who, in 1872, had been married to M. Gasser, moved to Lille, and dark pages began to be turned in the great worker's life-history. He no longer possessed the blessed gift of independence, which he had hoped to increase by his transfer to the north of France. He was perpetually interfered with by the priestly directors of the new house of learning, and what between worry and work, his hands were soon so full that the time was opportune for his influence to be undermined at the Academy of Science in Paris, where, thanks to Pasteur, the very name "microzyma" was rendered almost anathema.

How contrary his destiny must have seemed to Pro-

fessor Béchamp! At the period when he was finally shaping his remarkable and exhaustive explanation of the processes of life, disease and disruption, unexpected opponents arose in the shape of priests, uninstructed in science, whose narrow minds could only find irreligion and materialism in views that, had they possessed any discernment, they would have realised could have combated far better than any of the dogmas of Rome the atheism which, at that epoch, was inclined to link itself with science. The "little learning," with its dangers, would have been revivified by the deeper draughts of Béchamp's profounder teaching. But of this, the Bishops and Rectors of Lille gleaned no idea in their complacent ignorance, and in diplomacy, perhaps, Béchamp fell far short of Pasteur. Subterfuge was impossible to him. He could not pretend that ignoramuses knew more than he did of the workings of Creation, and he made no attempt to defer to the bigoted clerics, since to do so would have savoured too much of bowing the knee to Baal. He was no opportunist, and the Creator he imaged, as portrayed by his marvellous works, outdistanced the anthropomorphic ideas of the priests as the God of the Israelites surpassed the crude man-made Philistine idols.

Worried though he was at every turn, the Professor continued to put into shape the conclusions derived from the ceaseless experiments he had undertaken at Montpellier and still pursued at Lille, regardless of all interruptions. The deeper he delved into the microzymian doctrine, the better it seemed to him were the answers it gave to the puzzles of contemporary science.

One of Béchamp's earlier achievements had been a close analysis of the albuminoids and a consequent discovery of their variations. Instead of finding them alike in each of the innumerable species of living beings, the Professor and his collaborators found them everywhere different, so much so that they could put no limit to them. This variety they proved by those precise chemical tests in the making of which Béchamp seems to have so utterly out-

stripped his contemporaries. They found that not only did the albuminoids vary in different species but also in the different organs of the self-same body. They thus found the differentiation between species and between the organs of the body to be due both to the individuality of the inherent microzymas and the dissimilarities of the albuminoids. For instance, in the hen's egg, they showed the complexity of the albumens that constitute the white and explained a method of separating these, while, from the yolk, they isolated the specific microzymas. Dr. Joseph Béchamp, the Professor's brilliant son, took a prominent part in carrying out these particular researches. He showed by a close analysis of eggs of every description that none of the albumens contained in either the white or the yolk is identically the same as that found in the egg of any other species. He made clearer than before the error of substantial unity. A fact, which his work made apparent, is that even chemically a creature is what it is in the very egg from which it issues, both by reason of the cytological elements and also the albumens. It had been thought that the albumen of secretions was the same as the albumen of the blood: not only did M. Joseph Béchamp discover this not to be a fact, but also that among those he isolated none possessed the same elementary composition as that of the serum. He showed that there exists a certain relation of cause and effect between the tissues through which the secretion passes and the nature of the albumens of the effusion. He thus disposed of Mohl's and Huxley's earlier views on the subject and of Claude Bernard's belief in a unique protoplasm. With his father he put forward manifold instances of the elemental differences between species. For example, they found that though the organisms of the mouth, that is, the microzymas, bacteria, epithelial cells, etc., resemble one another in form in man, in the dog, in the bull, in the pig, yet their chemical functions are very different. M. Joseph Béchamp showed that the microzymas even of the same gland in the same animal vary according to age and condition. His father

demonstrated the similarity he had found in the structure of the pancreas to that of the parotis and the dissimilarity in their products; while the secretions of the parotis he found to be different in man, horse and dog. The great teacher explained that it is owing to the microzymas of allied species of animals being often functionally different in certain of their physiological centres that each animal has diseases peculiar to it and that certain diseases are not transmissible from one species to another and often not from one individual to another even of the same species. Infancy, adult age, old age, sex, have their share in influencing susceptibility to disease-conditions.

These researches of the School of Montpellier certainly seem to throw light upon the nature of infection and on the immunity constantly met with, in spite of alleged exposure, from all kinds of infectious maladies. The world might have been spared the propagation and inoculation of disease-matters had the profound theories of Béchamp been followed instead of the cruder fashionable germ-theory of disease, which appears to consist of distorted half-truths of Béchamp's teaching.

Another special study of the younger Béchamp was to trace microzymas in the fœtus and in the organs of the body after birth, where by laborious experimentation he proved their varying multiplicity at different stages. He also showed the variations of their action in different organs, the placenta, liver, etc., and their variations of action at different ages, comparing, for instance, those of the fœtus with those of the adult, and demonstrating that no extraneous organisms could effect these changes. He also assisted his father in his researches on corpses, where the two Béchamps maintained that the inherent microzymas, apart from the assistance of foreign "germs," bring about decomposition. They taught that when the corporate life of a being is at an end, the infinitesimal organisms that originally built up its cells continue to flourish and by their life-processes destroy the habitat of which they were the up-builders. In 1880, Joseph Béchamp, as

indefatigable a worker as his father, demonstrated the presence of alcohol in tissues shortly after death and its disappearance in advanced putrefaction, when he considered it to be destroyed by a continuance of fermentation due to the very microzymas that had produced the alcohol in the first instance. Thus he explained the continued vitality of the organisms that had till lately vitalised the now inert corpse or carcass, and showed that "nothing is the prey of death; everything is the prey of life," to quote Antoine Béchamp's epigrammatic definition.

What a different future might have awaited the microzymian doctrine had life been spared to Professor Estor and to Joseph Béchamp, instead of both being cut off in the prime of manhood. But the inscrutable decrees of Providence dealt hardly with the great Master. His patriotic work foiled by bigotry, his scientific discoveries stifled by jealousy, his collaborators struck down by death, which spared neither his wife nor the young daughter of whom the priests had robbed him, he finally made his solitary way to Paris to find his chief detractor enthroned as the idol of the public, his own genius almost unrecognised. It was a dreary outlook and might easily have daunted even a brave spirit; but Béchamp's willpower rose indomitably to meet the future, and, aided and quickened by his splendid health and vitality, spurred him on to fresh investigations. With increasing years his incessant work never abated and he persevered in searching the mysteries of life-processes. Up to 1896 he continued to publish articles on milk, its chemical composition, its spontaneous changes and those occasioned by cooking. He not only maintained his early idea of its inherent autonomous microzymas, but he showed the distinctive character of various milks, human, bovine, etc. He denied the popular belief in milk being an emulsion, but was of the opinion, in which Dumas concurred, that the milk-globules are vesicles of a cellular type, that is, furnished with envelopes, which prevent their ready solution in ether in the milk stage, and in cream are causative of the clotting.

The crowning achievement of Béchamp's laborious and persecuted career was the publication, when in his eighty-fifth year, of a work on *The Blood* in which he applied his microzymian views to its problems, especially that of its coagulation. We cannot do better than quote Dr. Herbert Snow's summary in the *New Age* of the 1st May, 1915:—

"It represents the blood to be in reality a flowing tissue, not a liquid. The corpuscles, red and colourless, do not float in a liquid, as is commonly thought, and as our senses indicate, but are mingled with an enormous mass of invisible microzymas—the mixture behaving precisely as a fluid would do, while under normal conditions. They are each clad in an albuminous envelope, and nearly fill the blood vessels, but not quite. Between them is a very small quantity of intracellular fluid. These microzymas, in their albuminous shells, constitute the 'molecular microzymian granulations'—the third anatomical element—of the blood.

"Directly the natural conditions of blood-life cease, and the blood is withdrawn by an incision in the vessels, these molecular granulations begin to adhere to each other very rigidly. By this adhesion the clot is formed, and the process of coagulation is so rapid that the corpuscles are caught within its meshes before they have time to sink to the bottom, as by their weight they otherwise would do. Then we have a second stage. The albuminous envelope of the granulation becomes condensed and shrinks. So the clot sinks *en masse*, and expels the intracellular liquor. Finally, in the third stage, the corpuscles are crushed by the contracting clot, and the red yield their colouring to the serum without. There is no such thing as fibrin *per se*. 'Fibrin is not a proximate principle, but a false membrane of microzymas.' "

"There is much," adds Dr. Snow, "in this ingenious explanation of a difficult and hitherto by no means satisfactorily solved problem, which seems to indicate, at any rate, to the present writer, that it is worthy of far closer

examination and consideration than it would appear to have received . . ."

But, surely, that is only what may be said for the whole of Béchamp's microzymian teaching, which, in its pathological relationship, we can, from his writings, sum up as follows:—

The microzyma is that which is primarily endowed with life in the organised being and that in which life persists after the death of the whole or in any excised part.

The microzyma being thus the fundamental element of corporate life, it may become morbid through a change of function and thus be the starting-point of disease.

Only that which is organised and endowed with life can be susceptible to disease.

Disease is born of us and in us.

The microzymas may undergo bacterial evolution in the body without necessarily becoming diseased.

In a diseased body a change of function in the microzymas may lead to a morbid bacterial evolution. Microzymas morphologically identical with and functionally different from diseased microzymas may appear without a microscopic distinction being possible.

Diseased microzymas may be found in the air, earth, or waters and in the dejecta or remains of beings in which they were once inherent.

Germs of disease cannot exist *primarily* in the air we breathe, in the food we eat, in the water we drink, for the diseased micro-organisms, unscientifically described as "germs," since they are neither spores nor eggs, proceed necessarily from a sick body.

Every diseased microzyma has originally belonged to an organism, that is a body of some sort, whose state of health was reduced to a state of disease under the influence of various causes, which determined a functional change in the microzymas of some particular centre of activity.

The micro-organisms known as "disease-germs" are thus either microzymas or their evolutionary bacterial forms that are in or have proceeded from sick bodies.

The microzymas exist primarily in the cells of the diseased body and become diseased in the cell itself.

Diseased microzymas should be differentiated by the particular group of cells and tissues to which they belong rather than the particular disease-condition with which they are associated.

The microzymas inherent in two different species of animals, more or less allied, are neither necessarily nor generally similar.

The microzymas of a given morbidity belong to one certain group of cells rather than to another, and the microzymas of two given species of animals *are not susceptible to an identical affection.*

Such, roughly summed up, are the propositions that form Béchamp's basis of pathology. Needless to say, he put none forward as an untried theory: each was founded upon exact experimentation and observation.

In spite of the hold of Pasteurian dogma over the Medical Faculty, scientific minds, here and there, confirm fragments of Béchamp's teaching, without knowledge of it, from their independent studies. In this connection may be quoted the evidence before the Royal Commission on Vivisection[1] of Dr. Granville Bantock, whose great reputation needs no comment.

"Bacteriologists," he said, "have discovered that in order to convert filth or dead organic matter of any kind into harmless constituents, Nature employs micro-organisms (or microbes) as her indispensable agents . . . In the modern septic tank, it is the action of the micro-organisms, whether ærobic or anærobic, that dissolves the sewage, and it is the continuous action of these microbes that converts all manurial matter into the saline constituents that are essential for the nutrition of plant life." After several examples, Dr. Bantock continued:—"The microbe in its relation to disease can only be regarded as a resultant or concomitant;" and after quoting many in-

[1] *Report of the Royal Commission on Vivisection*, Q. 14,545-6 of the 4th Report, 1906, p. 77b.

stances of error of diagnosis through reliance on bacterial appearances, he queried:—"Is it not therefore reasonable to conclude that these micro-organisms . . . are certainly *not* causative of disease?" He also said:—"I am bound to accept as a matter of fact the statements made as to the association of the 'Loeffler bacillus' with diphtheria; but to say that their presence is the *result* of the disease appears to me to be the more sound reasoning."

Then again, we may quote the practical observations of the great pioneer of nursing, Florence Nightingale.

"Is it not living in a continual mistake," she said,[1] "to look upon diseases, as we do now, as separate entities, which *must* exist, like cats and dogs, instead of looking upon them as conditions, like a dirty and clean condition, and just as much under our own control; or rather, as the reactions of kindly Nature against the conditions in which we have placed ourselves? I was brought up by scientific men and ignorant women distinctly to believe that smallpox was a thing of which there was once a specimen in the world, which went on propagating itself in a perpetual chain of descent, just as much as that there was a first dog (or a first pair of dogs), and that smallpox would not begin itself any more than a new dog would begin without there having been a parent dog. Since then I have seen with my eyes and smelt with my nose smallpox growing up in first specimens, either in close rooms or in overcrowded wards, where it could not by any possibility have been 'caught,' but must have begun. Nay, more, I have seen diseases begin, grow up and pass into one another. Now dogs do not pass into cats. I have seen, for instance, with a little overcrowding, continued fever grow up, and with a little more, typhoid fever, and with a little more, typhus, and all in the same ward or hut. For diseases, as all experience shows, are adjectives, not noun substantives."

It was she who said also:—"The specific disease doctrine is the grand refuge of weak, uncultured, unstable minds,

[1] *Notes on Nursing*, p. 19 (note).

such as now rule in the medical profession. There are no specific diseases: there are specific disease-conditions."

Such was her teaching based upon far-reaching personal experience, upon opinions that are understandable in the light of Béchamp's microzymian doctrine, which thus gains confirmation from Nature's every-day lessons. It seems that causative disease-entities must give place to disease-conditions following upon bad heredity, bad air, bad food, vicious living and so forth, and, provided our ancestry be good, our surroundings sanitary and our habits hygienic, our physical status lies chiefly in our own keeping, for good or evil, as our wills may determine. Instead of being at the mercy of extraneous enemies, it rests principally with ourselves whether our anatomical elements, the microzymas, shall continue on the even tenor of their way, when our conditions will be those of health, or, from a change of environment in their immediate surroundings, develop morbidly, producing bad fermentative effects and other bodily calamities. Thus, while our own shortcomings are first reflected on them, so their ensuing corruption afterwards revenges itself upon us.

It has been argued in answer to Miss Nightingale's sound reasoning that she was only a nurse and therefore not qualified to express medical opinions. This objection comes oddly from the devout adherents of men, such as Jenner, who bought his medical degree for £15, and Pasteur, who managed to obtain by a majority of just one vote a place among the Free Associates of the Academy of Medicine! Let us, however, turn to the opinions of two genuine medical men and see how exactly they bear out the views of the great nurse. In the eighteenth chapter[1] of *The Wonderful Century*, a work by the great scientist, Professor Alfred Russel Wallace, we find that he quotes the medical statistician, Dr. Farr, and Dr. Charles Creighton, greatest of epidemiologists:—

[1] This chapter no longer appears in the work, but was formerly to be obtained separately from George Allen & Unwin, Museum St., London, W.C.

"In his (Dr. Farr's) Annual Report to the Registrar-General in 1872 (p. 224), he says:—'The zymotic diseases replace each other; and when one is rooted out, it is apt to be replaced by others which ravage the human race indifferently whenever the conditions of healthy life are wanting. They have this property in common with weeds and other forms of life: as one recedes another advances.' This substitution theory is adopted by Dr. Creighton, who, in his *History of Epidemics in Britain*, suggests that plague was replaced by typhus fever and smallpox; and, later on, measles, insignificant before the middle of the seventeenth century, began to replace the latter disease."

It is interesting that the replacement of disease-conditions noted by Florence Nightingale in unhealthy huts or wards, according to their changing degree of unhealthiness, exactly bears out what Dr. Charles Creighton shows to be the testimony of historic records. And this evolution or retrogression, as the case may be, of disease-conditions is surely explained by Béchamp's microzymian doctrine, which teaches that upon the anatomical elements, whether called microsomes or microzymas, the actual builders of the body-cells, depends our state of well-being or otherwise, and that a morbid change of function in these may lead to disease-conditions in us, the latter altering as the former varies, and the former influenced by surrounding conditions, whether insanitary or unhygienic.

If the microzymian teaching thus sheds light upon zymotic mysteries, how much more upon hereditary tendencies, too much overlooked by modern medical orthodoxy. Since the microzymas perpetuate life from parent to child, so they carry with them parental characteristics for good or evil, which may lie dormant throughout generations, or be made manifest, according to the microzymas that carry the preponderating influence, thus explaining the Laws of Mendel. Yet again, disease-conditions due to abnormal growth, of which cancer is an obvious example, seem to bear out Béchamp's doctrine

that upon the status of the microzymas depends the status of the whole or any part of the corporate organism.

In place of the modern system of treating that phantom shape, a disease-entity, and trying to quell it by every form of injection, scientific procedure on Béchamp's lines will be to treat the patient, studying his personal idiosyncracies. For these depend upon his anatomical elements, the microzymas, which, according to Béchamp, build up his bodily frame, preserve it in health, disrupt it in disease, and finally when the corporate association is ended by death, these, with or without extraneous help, demolish their former habitat, themselves being set free to continue an independent existence in the earth, the air, or the water in which they happen to find themselves. Any morbidity, which may be in them or in their evolutionary bacterial forms, is quickly dispelled by fresh air. And since the microzymas of different animals, different plants and different organs, lungs, kidneys, colon, as the case may be, are themselves all different, so will there be variation in their bacterial development, and so the innumerable forms of bacteria perceived everywhere are readily accounted for. As the British Empire, or the United States of America, or the Republic of France, are composed of innumerable varying individuals, so the corporate body of plant or animal is an association of living entities; and as the work of myriad individuals composes the life-processes of the nation, so the action of the microzymas constitutes the life-processes of all corporate beings.

What might not the new outlook on life and disease have been had Béchamp's belief been developed instead of stifled under the jealousy of a rival!

And now we will turn to some modern views that seem to bear out his teaching.

CHAPTER XIV

Modern Confirmations of Béchamp

As we have claimed that Béchamp laid the foundations of cytology, or the science of cellular life, it may be as well to give examples of modern views that bear out his early conclusions. For this purpose we cannot do better than quote the Presidential Address to the Zoological Section of the British Association for the Advancement of Science at Manchester in 1915 by Professor E. A. Minchin, M.A., Hon. Ph.D., F.R.S.

As we have seen, Béchamp combated Virchow's view of the cell as the anatomical unit, and did this in the sixties of the nineteenth century.

What is Professor Minchin's opinion in the year 1915?

"Many cytologists appear indeed to regard the cell, as they know it in the Metazoa and Metaphyta, as the beginning of all things, the primordial unit in the evolution of living beings. For my part, I would as soon postulate the special creation of man as believe that the Metazoan cell, with its elaborate organisation and its extraordinary perfected method of nuclear division by karyokinesis, represents the starting-point of the evolution of life."

Thus, after the lapse of more than half a century, we find this expert confirmation of Béchamp's teaching.

While Professor Béchamp and Professor Estor were working together, they were struck by seeing the granules, the microzymas, in cells associate and thread-like forms develop. There seems little doubt that, all those years ago, they were already observing different stages in that complicated series of changes, known as Karyokinesis or Mitosis, which occur in the division of the cell-nucleus, in which is effected an equal division of the substance of the nucleus of the parent-cell into the two new resultant nuclei.

This process, the chief phenomenon in the cleavage of a cell, is the mode of cell-multiplication for the up-building of those structures known as the bodies of all living species. According to the most popular modern view, it is effected by the granules which, on uniting, are known as chromatin threads, the name "Chromatin" being applied to their substance because of the deeper shade it takes when stained for observation under the microscope. Staining methods greatly facilitate, although they occasionally falsify, the work of present-day observers; but these were but little known in the middle of the last century, so that Béchamp must have been far ahead of his generation in his manner of microscopically investigating the intricacies of cellular life and in viewing phenomena not yet noticed by his contemporaries. That early axiom of his that minute living granules build up cells holds good to-day, more than half a century later, regardless of nomenclature. Indeed, when we come to names, the number and variety in use are sufficient to befog any clearness in the matter, and the pity seems that general use has not been made of Béchamp's comprehensive term, "microzyma." In regard to Béchamp's priority in demonstrating the rôle of the granulations and the subsequent confusion of terminology, we may quote M. Nencki, a Swiss Professor of Medical Chemistry at Berne :—[1]

"To my knowledge it is A. Béchamp who was the first to consider certain molecular granulations, which he named microzymas, to be organised ferments, and that he defended his view resolutely against various attacks."

In making his own acknowledgment of the molecular granulations of the pancreas, M. Nencki continues:—"These are evidently the *microzymas* of Béchamp, the *coccus* of Billroth, the same thing as the *monas crepusculum* of Ehrenberg."

The outstanding names for the minute dots, present in cell-substance and distinguishable under the microscope,

[1] *Gesammelte Arbeiten I.* p. 212. (1904).

are, when arranged in chronological order, "molecular granulations," "microzymas," "microsomes," or "chromatin granules."

Call them which you will, it was these Béchamp intended when he wrote[1]:—"The cell is a collection of little beings, which have an independent life, a special natural history."

Professor Minchin, in his Presidential Address, without, however, rendering any acknowledgment to Béchamp, echoes his opinion:—"To each such granule must be attributed the fundamental properties of living organisms in general; in the first place, metabolism, expressed in continual molecular change, in assimilation and in growth, with consequent reproduction; in the second place, specific individuality."

This was exactly Béchamp's teaching, and moreover, he showed that the microzymas are the transmitters of heredity. According to him, a plant or an animal is what it is by virtue of its microzymas. These are the link between the animal and vegetable kingdoms. Though appearing intrinsically the same, yet it is they that differentiate the substance of one living being from that of another. It is by reason of its microzymas that an acorn develops into an oak, a hen's egg into a chicken; microzymian influence decides the child's likeness either to father or mother. And here again we find the confirmatory modern view that in the chromatin lies the secret of heredity.

Professor MacBride[2] thus bears out the opinion of Béchamp:—"There seems to be no escape from the position that the chromatin, viewed as a whole, is the bearer of the hereditary tendencies, for the influence of the father in determining the character of the offspring is as potent as that of the mother. Now the head of the spermatozoon is the only part of the father that enters into the constitu-

[1] *Comptes Rendus de l'Académie des Sciences* 66, p. 859. *Les Microzymas*, p. 972. (Appendix).

[2] Section D. Reports of British Association, 1915. *Discussion on the Relation of Chromosomes to Heredity*, by Professor E. W. MacBride, F.R.S.

tion of the progeny, and this appears to consist practically exclusively of chromatin. May not the chromosomes be simply groups of these determiners (of characteristics, qualities, etc.) adhering by mutual chemical affinity under the peculiar chemical conditions obtaining in the cell in the period preceding karyokinesis? If this be the case, the apparent total disappearance of chromosomes during the resting period could be accounted for."

It is possible that for want of modern appliances, Béchamp may have overlooked the great importance of the cell nucleus in his cellular doctrine; but, even so, Professor Minchin confirms the correctness of his view in ascribing the supreme influence to what we may indifferently term the microzymian, granular or chromatinic entities.

"Already," says Professor Minchin, "One generalisation of cytologists has been torpedoed by the study of the Protista" (a very primitive form of micro-organism). "The dictum '*omnia nucleus e nucleo*' is perfectly valid as long as it is restricted to the cells of Metazoa and Metaphyta, to the material, that is to say, to which the professed cytologist usually confines his observations. But in the Protista, it is now well established that nuclei can arise *de novo*, not from pre-existing nuclei, but from the extra-nuclear chromatin for which Hertwig first coined the term 'chromidia.' "

Let us run through Béchamp's early views as we find them expressed in his *Théorie du Microzyma:*—[1]"Microzymas are builders of cells, and, by evolution, become vibrios: they are histologically active; they are producers of zymases (ferments): they are physiologically active; and in noting that zymases are agents endowed with a chemical activity of transformation or decomposition, it may be said that microzymas can generate chemical energy; it is thanks to the microzymas that we digest and that we are able to transform and assimilate the materials that serve to nourish us. They are thus *chemically* active; placed in certain artificial surroundings, called putrescible,

[1] p. 319.

under favourable circumstances, they bring about decomposition (that is fermentation), in other words, they nourish themselves while they multiply, no matter whether they evolve into vibrios or whether they do not do so. They are therefore individually organisms comparable to those we call living and organised ferments, etc., etc. Finally, they defy putrefaction, and if I add that they are not digested in the condition of animal matter where they are, one can say that *they are physiologically indestructible*."

Now let us compare the modern views of Professor Minchin:—"I regard the chromatin elements as being the constituents which are of primary importance in the life and evolution of living organisms mainly for the following reasons: the experimental evidence of the preponderating physiological rôle played by the nucleus in the life of the cell; the extraordinary individualisation of the chromatin particles seen universally in living organisms and manifested to a degree which raises the chromatinic units to the rank of living individuals exhibiting specific behaviour, rather than that of mere substances responsible for certain chemico-physical reactions in the life of the organism; and last, but by no means least, the permanence and, if I may use the term, *the immortality of the chromatinic particles in the life-cycle of organisms generally*."

Here it may be objected that though Professor Minchin confirms Professor Béchamp's views as regards the individuality and immortality of the minute cellular granules, no confirmation is given of vibrionic, or as one would say more familiarly, bacterial evolution.

Yet the modern Professor has no hesitation in enunciating such a belief, if relegated to primeval eras and the realm of hypothesis and fancy, imagining the development of living forms from the earliest living beings, "minute, possibly ultra-microscopic particles of the nature of chromatin." "These earliest living things," he says, "were biological units or individuals which were the ancestors, in a continuous propagative series, of the chromatinic

germs and particles known to us at the present day as universally-occurring constituents of living organisms." Moreover, he tells us:—"The evolution of living things must have diverged in at least two principal directions. Two new types of organisms arose, one of which continued to specialise further in the vegetative mode of life, in all its innumerable variations, while the other type developed an entirely new habit of life, namely, a predatory existence. In the vegetative type the first step was that the body became surrounded by a rigid envelope. Thus came into existence the bacterial type of organism." Here is confirmation of belief in bacterial evolution from chromatinic, otherwise microzymian, granules, further supported by such statements as:—"I agree with those who derive the bacteria as primitive, truly noncellular organisms, directly from the biococcus (Mereschkowsky's term) through an ancestral form."

It is curious to compare this expert readiness of belief in a primeval evolution, a matter of pure conjecture, with the indifference displayed towards Béchamp's experimental demonstrations of bacterial development. In regard to this, we may quote his opinion as follows:—[1]"But you must not imagine that the microzymas are converted into bacteria without any transition: on the contrary, there are many intermediate forms between the microzymas and the bacteria. What you must bear in mind is that the medium has a great influence on the appearance of the various forms in their evolution from the microzymas and that there is an infinity of species which vary in their function; finally, that according to the nature of the medium, the microzymas can produce cells in place of bacteria, true cellular microphytes, and from these the moulds."

It has been argued that modern research has not confirmed Béchamp's statement:—[2]"We have seen the microzymas of animal cells associate two by two, or in larger

[1] *Les Microzymas*, p. 140.
[2] *Les Microzymas*, p. 972 (Appendix).

numbers, and extend themselves into bacteria." But it must be remembered that other declarations of Béchamp's, strenuously combated, have since met with confirmation. Take, for instance, his claim that bacteria could change their forms, the rod-shape pass into the spheroid, etc. This was denied by Pasteur. None the less, after the passing of years, a worker at the very Institute that bears the latter's name has confirmed Béchamp's statement.

We may recall the prominence given in London papers to what was styled an "Important Discovery by a French Lady Scientist." The *Daily News* of the 8th April, 1914, provides a simple summary:—

"PARIS, *Tuesday*, *March*, 31."

"Mme. Victor Henri, the lady bacteriologist, has made one of the most important discoveries in that branch of research for many years. She has, by subjecting bacteria to the action of ultra-violet rays, succeeded in creating a new species of bacteria from a species already known. The experiment was made with the anthrax bacillus, which from a rod-shape was transformed into a spherical coccus."

Thus another contention of Professor Béchamp's meets with modern substantiation. And more than this the statement that he saw microzymian evolution bring about the formation of primitive organisms is at the present day being confirmed by an acknowledged student of his, a Frenchman, named Galippe. The following account of his work has been kindly summarised for us by Mr. E. J. Sheppard, a cytologist who formerly carried out some researches in connection with the late Professor Minchin and who himself is conversant with and subscribes to much of Béchamp's teaching.

"NORMAL PARASITISM AND MICROBIOSIS"

"Galippe[1] describes experiments with fruits and animal tissues which confirm the assumption of the existence of various parasites in the normal tissues of the vegetable and animal kingdom.

[1] *Bull. de l'Académie de Méd.*, Paris, July, 1917, No. 29, pp. 30-76.

"But besides this more or less accidental normal parasitism, he says, there is another order of facts, more general, more constant, and dominating to a certain extent the life of the tissues, namely, the presence in the cell itself of living elements, elements indispensable to its functional activity.

"He accepts Béchamp's term of 'microzyma' for these, and calls the manifestations of the biological activity of these intracellular elements, 'microbiosis.'

"These infinitesimal elements may survive the destruction of the cell, and they may acquire forms and biological properties that they previously did not possess. They may function in a kind of autonomous manner and may adapt themselves to the new conditions in which they find themselves and continue their evolution.

"The normal parasitism and the microbiosis may continue their evolution parallel to or independently of each other.

"In his experiments with apples, etc., Galippe relates that he was able to induce the appearance of micro-organisms from the microbiosis while excluding those from normal parasitism. The methods by which he realised this included mechanical trauma, contusions, etc., and he thus was able to trace certain manifestations of intracellular life and observe the appearance and evolution of certain living elements and cultivate them further.

"These facts of general biology are applicable to all tissues, he says, all cells, whatever their origin. The most striking example is in war wounds. The crushed tissues in the wounds favour the development of the phenomena due to microbiosis. The danger from leaving these contused tissues in the wounds is recognised now by all surgeons and the surgical cleansing of all wounds is now the routine practice.

"What they do not know, and what Galippe devotes the fifty pages of his monograph to prove is that on account of the normal parasitism and the microbiosis, the part played by the crushed tissues and the more extravasated blood is at the same time more important and more decisive. They may give birth directly, without foreign collaboration, to infectious elements, so that an absolutely aseptic projectile is capable of infecting a wound solely by its mechanical action in starting the abnormal evolution of the living intracellular elements already present.

"The research was undertaken in Landouzy's laboratory, and the data presented corroborate the lessons already learned from clinical observation."

In *The Vaccination Inquirer* for December 1st, 1920, Mr. Alexander Paul summarises from the Reports of the French

Academy of Science[1] the results of other observations by M. V. Galippe of living microzymas and their modification into bacilli. Mr. Paul quotes the latter as follows:—"Now the microzymas form an integral part of the cell and cannot confer on the tissues a septic character which they do not themselves possess when they belong to a healthy organism. In spite of some failures, due without doubt to accidental causes, the brilliant results obtained in surgery by the process of grafting are an irrefutable proof of this. The grafts are not dead in the absolute sense of the word since they contain living elements capable of evolution *in situ*, or in the midst of appropriate cultures, as demonstrated by our experiments. Neither glycerine, nor alcohol, nor time destroy the microzymas of the tissues. These different agents can only diminish or suspend their activity. They are endowed with perennial life."

Mr. Paul refers to another Communication by M. Galippe to the Academy of Science[2] on "Living Micro-organisms in paper: their Resistance to the Action of Heat and of Time." In this the modern worker treats of cultivable elements found in all paper, even in ancient Chinese manuscripts and Egyptian papyrus, which have yielded micro-organisms endowed with movement.

Mr. Paul subsequently quotes Galippe's *résumé* of his research on flowers:[3] "Reviewing this long series of experiments, the fact that we have set forth shows that the living part of the protoplasm is constituted of microzymas."

Finally, Mr. Paul refers to Galippe's discovery of microzymas in amber, and himself comments:—"How sad to think that M. Béchamp, after his valiant struggles till a ripe old age with Pasteur and his school, whom he accused of perverting his discoveries and building upon them a false microbian hypothesis, should have gone down to the grave without enjoying the satisfaction of hearing

[1] *Comptes Rendus*, September, 1919.
[2] *Comptes Rendus*, November 3, 1919.
[3] *Comptes Rendus*, February 9, 1920.

that later research has established his position, and seeing the too long tabooed name 'microzyma' reinstated in the records of the Academy of Science!"

Many who seem never to have heard of Béchamp, appear to be working slowly and laboriously towards his view. We may quote, for example, a passage from page 64 of *Health, Disease and Integration*, an interesting and advanced work by H. P. Newsholme, M.A., M.D., F.R.C.P., D.P.H., Medical Officer of Health for the City of Birmingham. "Thus we again reach a position," writes Dr. Newsholme, "in which, while not negating (*sic*) the rôle played by an extraneous virus in producing encephalitis lethargica, we nevertheless find reason for not rejecting the possibility that a purely natural enzyme or 'virus,' produced by the individual and not by any bacteria harboured by him or introduced from outside, may on occasion be the cause of particular cases of a syndrome indistinguishable from that arising from extraneous infection."

In conclusion we may say that not only have we evidence of modern confirmation of Béchamp's views, but indications are many that his explanation of cellular and micro-organic life will receive a warm welcome from disinterested, unprejudiced inquirers. For instance, we may quote from a work published in 1918, entitled *Philosophy of Natural Therapeutics*,[1] by Henry Lindlahr, M.D.

"Until a few weeks ago," writes Lindlahr, "I was not aware of the fact that a French scientist, Antoine Béchamp, as far back as the middle of the last century, had given a rational, scientific explanation of the origin, growth and life activities of germs and of the normal living cells of vegetable, animal and human bodies. This information came to me first in a pamphlet entitled *Life's Primal Architects*, by E. Douglas Hume.[2] . . . According to the

[1] It appears that, since the death of Henry Lindlahr, all references to Béchamp have been eliminated from later editions of the *Philosophy of Natural Therapeutics*.

[2] Chapter X of the first edition of *Philosophy of Natural Therapeutics* is, for the most part, a reprint of portions of *Life's Primal Architects*.

teachings of Béchamp, cells and germs are associations of microzymas. The physical characteristics and vital activities of cells and germs depend upon the soil in which their microzymas feed, grow and multiply. Thus microzymas, growing in the soil of procreative germ plasm, develop into the normal, permanent, specialized cells of the living vegetable, animal or human organism. The same microzymas feeding on morbid materials and systemic poisons in these living bodies develop into bacteria and parasites. . . . How wonderfully the discovery of microzymas confirms the claims of Nature Cure philosophy, according to which bacteria and parasites cannot cause and instigate inflammatory and other disease processes unless they find their own peculiar morbid soil in which to feed, grow and multiply! . . . Knowledge of the researches and teachings of Béchamp came to me but recently, after the manuscript of this volume had been practically completed. It was most gratifying to discover at the last moment this missing link which corroborates so wonderfully my own experience and teachings. . . . What a wonderful correspondence this theory of the origin of cell life bears to the latest scientific opinions concerning the constitution of the atom! As all elements of matter and their atoms are made up of electrons vibrating in the primordial ether, so all cells and germs are made up of microzymas. As the electrons, according to their numbers in the atom and their modes of vibration, produce upon our sensory organs the effects of various elements of matter, so the microzymas, according to the medium or soil in which they live, develop into various cells and germs, exhibiting distinctive structure and vital activities. Modern biology teaches us that all permanent, specialized cells present in the complicated adult body are actually contained in the original procreative cell which results from the union of the male spermatazoon and the female ovum. Science, however, has failed to explain this seeming miracle—how it is possible that all the permanent cells of the large adult body can be present from the beginning in

the minute procreative cell and in the rudimentary body of the fœtus. Béchamp's theory of microzymas brings the rational and scientific explanation. If these microzymas are as minute in comparison to the cell as the electrons are in comparison to the atom, and the atom in comparison to the visible particles of matter, then the mystery of the genesis of the complex human body from the procreative cell, as well as the mysteries of heredity in its various phases, are amenable to explanation. If the microzymas are the spores, or seeds, of cells, it is possible to conceive that these infinitesimal, minute living organisms may bear the impress of the species and of racial and family characteristics and tendencies, finally to reappear in the cells, organs and nervous system of the adult body."

Just as Dr. Lindlahr has accepted Béchamp's microzymian doctrine as the explanation of pathogenic and other mysteries, so we cannot but anticipate a similar acceptance on the part of other workers, and considerable advance, as an ever-widening circle claims acquaintance with Béchamp's epoch-making discoveries. We would repeat the prophecy of the *Moniteur Scientifique* that time will do justice to his work and make it known in its entirety. And with this end in view we would advise all students to go direct to the writings of this brilliant Frenchman who, even in that epoch of intellectual giants, is seen in perspective to have been an out-standing genius of the nineteenth century!

PART THREE

THE CULT OF THE MICROBE

CHAPTER XV

The Origin of "Preventive Medicine"

It was at the commencement of the year 1873 that Pasteur was elected by a majority of one vote to a place among the Free Associates of the Academy of Medicine. His ambition had indeed spurred him to open "a new era in medical physiology and pathology," but it would seem to have been unfortunate for the world that instead of putting forward the fuller teaching of Béchamp, he fell back upon the cruder ideas now popularly known as the germ-theory of disease. It is astonishing to find that he even used his powerful influence with the Academy of Science to anathematize the very name of "microzyma," so much so that M. Fremy, the friend of Béchamp, declared that he dared not utter the word before that august assemblage.[1] As a name was, however, required for air-borne micro-organisms, Pasteur accepted the nomenclature "microbe" suggested by the surgeon, Sédillot, a former Director of the Army Medical School at Strasbourg. The criticism might be passed that this term is an etymological solecism. The Greeks used the word *Macrobiorus* to denote races of long-lived people, and now a name, concocted from Greek words for short-lived, was conferred upon micro-organisms, whose parent-stem, the microzyma, Béchamp had described as "physiologically imperishable." Man, who so seldom lasts a century, might better be called a microbe, and the microzyma a macrobe!

It was not till 1878 that Sédillot put forward his sug-

[1] *Le Sang, par A. Béchamp,* Preface, p. 43, note.

gestion; but before this, Pasteur had been busy nominating micro-organisms as direct agents of varying troubles, and in 1874 he was gratified by an appreciative letter from Lister. The latter wrote that the Pasteurian germ-theory of putrefaction had furnished him "with the principle upon which alone the antiseptic system can be carried out."[1]

However, let us turn to that verdict of time, which, according to Pasteur's own dictum, must pronounce judgment on a scientist. Before the last Royal Commission on Vivisection, which sat from 1906 to 1908, Sir Henry Morris, President of the Royal College of Surgeons, wishing to make out the best case that was possible for Pasteur, had, all the same, to acknowledge:—"In consequence of further researches and experience some modification of the technique first introduced by Lord Lister occurred,[2] and the evolution of the aseptic method resulted."

Dr. Wilson points out[3] in his Reservation Memorandum of the Royal Commission, that "the basis of aseptic surgery, which in essence is clean surgery, was laid, as stated in the Report and in reply to a question by Sir William Collins, by Semmelweiss before 1850, who attributed the blood-poisoning which devastated his lying-in wards in a Viennese hospital to putrid infection and strongly urged cleanliness as a means of preventing it." Dr. Wilson shows how Lord Lister brought about the application of this advice as to cleanliness considerably before his ideas were moulded by Pasteur. This latter influence, this Pasteurian "Theory that the *causa causans* of septicism in wounds rested on micro-organisms in the air was an altogether mistaken theory."[4] It was on this "mistaken theory," this "principle," provided for him by Pasteur, that Lord Lister based his use of the carbolic spray, of which, before the Medical Congress in Berlin, in 1891, he

[1] *The Life of Pasteur*, by René Vallery-Radot, p. 238.
[2] *Final Report of the Royal Commission on Vivisection*, p. 25.
[3] p. 89.
[4] p. 90.

made the honest recantation:—"I feel ashamed that I should ever have recommended it for the purpose of destroying the microbes in the air." Thus pronounces the verdict of time against the theories of Pasteur; while, as regards the teaching of Béchamp, what do we find? Dr. Wilson continues:—"The real source of all the mischief was the unclean or putrefying matter which might be conveyed by hands, dressings, or other means, to freshly made wounds." Such contamination is exactly explained by the microzymian doctrine, which teaches that this putrefying matter with its morbid microzymas might affect the normal condition of the inherent microzymas of the body, with which it comes into contact. Thus the verdict of time corroborates Béchamp.

Pasteur declared danger to arise from atmospheric microbes. He talked of "invaded patients," and triumphantly chalked upon a blackboard the chain-like organism that he called the germ of puerperal fever.

Béchamp maintained that in free air even morbid microzymas and bacteria soon lose their morbidity, and that inherent organisms are the starting points of septic and other troubles.

What was Lord Lister's final judgment after having abandoned the method into which he was misled by Pasteur?

We give it in his own words as quoted by Dr. George Wilson[1]:—"The floating particles of the air may be disregarded in our surgical work, and if so, we may dispense with anti-septic washing and irrigation, provided always that we can trust ourselves and our assistants to avoid the introduction into the wound of septic defilement from *other than atmospheric sources*."

Comment is unnecessary.

But in the seventies of the nineteenth century, the specific air-borne germ-theory had the charm of novelty and its crude simplicity attracted the unscientific, although many

[1] See Dr. G. Wilson's *Reservation Memorandum of the Royal Commission on Vivisection*, p. 90.

scientists opposed it sturdily. Pasteur, however, continued upon a triumphal career of pronouncements upon disease-germs and was largely assisted by the conclusions of Dr. Koch and other workers. Anthrax, to which we have already alluded, offered him a convenient field for his quest of the microbe, and a little later his attention was turned to an organism, first noticed by an Alsatian surgeon, named Moritz, and afterwards arraigned by Toussaint for inducing chicken-cholera. This so-called microbe Pasteur cultivated assiduously, as he had already cultivated the *bacillus anthracis*. He also inaugurated the fashion for what may be called the study of artificial disease-conditions; that is to say, instead of giving attention to Nature's experiments in naturally diseased subjects, human and animal, the mania was aroused for inducing sickness by poisonous injections, a practice Pasteur started about this time, and which his followers have so persistently copied that some have even deliberately performed iniquitous experiments upon men, women and children. There can be no question that, since his day, bird and animal victims of every species have languished by millions all over the world in pathological laboratories, and that had Pasteur never lived, our "little brothers and sisters," to quote St. Francis of Assisi, would have been spared incalculable agonies.

His admirers will, of course, retort that his experiments were undertaken with a direct view to alleviate suffering, and, in the first instance, animal sicknesses, particularly splenic fever. But it must strike anyone as a topsy-turvy method to start the cure of natural diseases by the production of artificial, and the principle of vicarious suffering can surely only hold good ethically by voluntary self-sacrifice. But we are not here so much concerning ourselves with the ethics of Pasteur's procedure as with its practical outcome, so let us turn our attention to the unfortunate hens that were numbered among his early victims.

Pasteur tested his cultures of the so-called chicken-

cholera microbe upon poultry and killed a number of birds with systematic regularity. It came about, however, accidentally, that a few were inoculated with a stale culture, and then merely sickened to recover. This did not, however, save them from further experiments and these already "used" hens were now given a fresh dose of new culture. Again they proved refractory to the death that had been designed for them. This immunity was promptly ascribed to the previous dosage of stale culture. Pasteur then started to inject attenuated doses into hens, and claimed thus to protect them from death when afterwards inoculated with fresh virus.

"Was not this fact," his biographer asks,[1] "worthy of being placed by the side of that great fact of vaccine over which Pasteur had so often pondered and meditated?"

His meditations, however, show nothing of the caution his biographer is so anxious to ascribe to him.

"Original researches," he says,[2] "new and bold ideas, appealed to Pasteur. But his cautious mind prevented his boldness from leading him into errors, surprises or hasty conclusions. 'That is possible' he would say, 'but we must look more deeply into the subject.' "

However, bold ideas had, apparently, only to have been made familiar by time for cautiousness to forsake Pasteur. A true disposition of scientific doubt would have prompted him to establish the truth of the success or failure of Jennerian vaccination before accommodating accidents or theories to account for it. As a matter of fact, Koch, in 1883,[3] would not admit that the chicken cholera prophylaxis had the value that was claimed for it; while Kitt, in 1886,[4] declared that ordinary precautions (cleanliness, isolation of infected birds, etc.), were preferable. In regard to the particular accident of the stale culture,

[1] *The Life of Pasteur*, by René Vallery-Radot, p. 300.

[2] *ibid.* p. 33.

[3] *Medical Press and Circular*, January 17, 1883. (Quoted in *Rabies and Hydrophobia* by Surg. General A. C. Gordon.)

[4] *Deutsche Zeitschrift für Thiermedicin*, December 20, 1886. (Quoted in Sternberg's *Text-book of Bacteriology*.)

which was made the foundation-stone for the whole system of inoculation, it is evident that, like most people, Pasteur had accepted vaccination without personal investigation, and so, like many others, showed himself possessed of a simple credulity that is the antithesis to scientific cautiousness. This criticism is the more justified because at this date in France, as in England, the subject of vaccination had entered the field of controversy. In 1863, Ricord, a famous French physician, was already delivering a warning against the transmittance of syphilis by the practice. By 1867, the Academy had received evidence of the truth of this contention; and in 1870 Dr. A. H. Caron, of Paris, declared that long since he had positively refused to vaccinate at any price.

It may be well to recall what happened when Dr. Charles Creighton was asked to write an article on vaccination for the *Encyclopædia Britannica*. He complied, but being a scientist in deed as well as in name, felt it incumbent first to study the subject. As a consequence, the article had to be condemnatory, for investigation proved vaccination to be "a grotesque superstition" in the opinion of the greatest of modern epidemiologists.

Pasteur, on the contrary, incautiously accepting the popular view, had a credulous belief in the success of vaccination, and made his hens' behaviour account theoretically for a practice that he seems never to have investigated historically. It is true that he paused to notice a discrepancy between Jenner's vaccination and the theory founded upon it. According to Pasteur, a previous injection of a stale culture safeguarded against a later injection of fresh virus; but, how could two such dissimilar disease-conditions as cowpox and smallpox be a protection the one from the other? "From the point of view of physiological experimentation," he said,[1] "the identity of the variola virus with the vaccine virus has never been demonstrated."

We are not engaged upon an anti-vaccinist treatise, but

[1] *The Life of Pasteur*, by René Vallery-Radot, p. 308.

as Jennerian vaccination, whether in its original form of cowpox, or its modernised guise of smallpox matter, passed (usually) through a heifer, is the foundation of Pasteurian inoculation, the two subjects are linked together and with the demolition of the first follows logically the downfall of the second. The whole theory is rooted in a belief in the immunity conferred by a non-fatal attack of a disease. The idea arises from the habit of regarding a disease as an entity, a definite thing, instead of a disordered condition, due to complex causes; the germ-theory of disease, in particular, being the unconscious offspring of the ancient Eastern faith in specific demons, each possessed of his own special weapon of malignity. Thus the smallpox inoculation introduced into England from Turkey by Lady Mary Wortley Montagu in the eighteenth century and its Jennerian substitute of cowpox inoculation were based on the ancient Indian rite of subjecting people to an artificially induced attack of smallpox to propitiate Sheetula-Mātā, the goddess of that torment.

Believers in the doctrine of immunity may correctly retort that seeming superstitions are often founded upon the observations of experience. Be that as it may, what remains for the lover of accuracy is to examine each superstitious belief upon its own merits and test the facts of life in regard to it. The assertion that because many people have had a one and only attack of any specific complaint, an auto-protection has thus been afforded them is, surely, no more scientific than the old Indian belief in the assuaging of the wrath of a malignant goddess. As Professor Alfred Russel Wallace says[1]:—"Very few people suffer from any special accident twice—a shipwreck, or railway or coach accident, or a house on fire; yet one of these accidents does not confer immunity against its happening a second time. The taking it for granted that second attacks of smallpox, or of any other

[1] *The Wonderful Century*, by Alfred Russel Wallace, LL.D., Dubl., D. C. L. Oxon, F.R.S., etc., chap. 18, p. 296. In recent editions of this book, chap. 18 is omitted owing to its former publication as a separate pamphlet.

zymotic disease, are of that degree of rarity as to prove some immunity or protection, indicates the incapacity for dealing with what is a purely statistical question."

Yet so imbued is medical orthodoxy with the immunity-theory that we recall a doctor[1] laying down the law on this subject even though his own daughter had recently died of a *third* attack of scarlet fever!

As Herbert Spencer has shown in his *Principles of Psychology*,[2] there is in the genesis of nerves a great likelihood of the development of habit. Common experience tells that there is a habit of taking cold and that complaints, such as influenza, are apt to be repeated. A trifling trouble, such as a cold-sore, may often be observed to reappear time after time in the same spot. If we wish to theorise, it might seem probable that when the system undergoes such a thorough upheaval as that brought about by serious disorders, like smallpox, the chance of recurrence is markedly less than in more trifling disturbances, such as colds and influenza. We have to remember that what we call disease is often Nature's method for ridding us of poisons, and, to take a homely example from household life, while house-cleaning takes place usually once a year, the dusting of rooms is of frequent occurrence. Such a theory is, however, palpably opposed to belief in immunity through artificially induced disorders, and, moreover, plausible though it may seem, it appears to be contradicted by statistical evidence. The testimony of Professor Adolf Vogt, who from 1877 to 1894 was Professor of Hygiene and of Sanitary Statistics in the University of Berne, Switzerland, is quoted by Professor Alfred Russel Wallace in Chapter Eighteen of *The Wonderful Century*. According to statistical data obtainable at his period, Vogt supplied a mathematical demonstration that a person who had once undergone smallpox was 63 per cent more liable to suffer from it again in a subsequent epidemic than a person who had never been a victim to it. Vogt concluded: "All this

[1] Dr. Alfred Salter.
[2] Vol. 1, p. 579.

justifies our maintaining that the theory of immunity by a previous attack of smallpox, whether the natural disease or the disease produced artificially, must be relegated to the realm of fiction." Certainly, if no auto-prophylaxy is induced by natural disorders, no claim can surely be made for auto-prophylaxy from artificially provoked disturbances.

In regard to vaccination against smallpox, experience can be our guide, since we have a whole century's history whereby to decide for or against its efficacy. We are faced by outstanding facts from among which we may quote an illustrative example provided by Professor Wallace in that eighteenth chapter of *The Wonderful Century*, which he tells us elsewhere is likely to gain in the future the verdict of being the most scientific of all his writings. In it he shows how free vaccination was provided for in 1840, the operation made compulsory in 1853, and in 1867 the Guardians were ordered to prosecute evaders, and so stringent were the regulations that few were the children who escaped vaccination. Thus the following table provides a striking illustration of the inefficacy of vaccination in regard to smallpox mortality.

Date	England and Wales Deaths from Smallpox
1857-59	14,244
1863-65	20,059
1870-72	44,840

Between	Increase per cent of population	Increase per cent of smallpox deaths
1st & 2nd epidemic	7	40.8
2nd & 3rd epidemic	9	123.0

We see here that while the population went up only 7 per cent and 9 per cent, smallpox mortality increased at the rate of 40.8 per cent and 123 per cent, and this in face of an ever-multiplying number of vaccinations!

Now let us turn to some military testimony, since in all countries the men of the army and navy are the most thoroughly vaccinated members of the community.

Under the date of January, 1899, Chief Surgeon Lippencott of the U.S.A. Army, writing from Manila, said: "The entire Command has been vaccinated at least four times since the appearance of the disease (smallpox)." In the following March, he wrote again to state that all danger was over. However, in the Reports of the Surgeon General of the U.S.A. Army are to be found the following figures of smallpox cases and deaths:

U.S.A. Army

Year	Cases	Deaths	Fatality-rate per cent
1899	267	78	29.21
1900	246	113	45·93
1901	85	37	43·53
1902	63	12	19.05

During the same period, the smallpox fatality-rate among the far less vaccinated general population of the United States did not exceed 3 per cent!

To turn back to *The Wonderful Century*,[1] Professor Wallace provides a comparison between the British Army and Navy and the unvaccinated inhabitants of Leicester during a period when the fighting forces on land and sea, at home and abroad, were admitted to have been "completely revaccinated." Leicester is taken as an example, because of the unvaccinated condition of almost all its inhabitants since the smallpox outbreak of 1871 and 1872. Before this, 95 per cent of the children born were vaccinated and the huge attack and death-rates during the epidemic were sufficient to prove the futility of vaccination. The authorities were, therefore, led to try improved sanitation and isolation as preventives, and have been rewarded not only in comparative freedom from smallpox, but also in the best health-rate of all the industrial towns of Great Britain. Professor Wallace writes as follows:—"The average annual smallpox death-rate of this town (Leicester) for the twenty-two years, 1873-94, inclusive, is thirteen per million (see 4th Report, p. 440); but in order to compare

[1] Chap. 18, pp. 284, 285.

with our Army and Navy we must add one-ninth for the mortality at ages 15-45 as compared with total mortality, according to the table at p. 155 of the *Final Report*, bringing it to 14.4 per million, when the comparison will stand as follows:—

		Per Million
Army (1873-94) smallpox death-rate	......	37
Navy „ „ „	......	36.8
Leicester „ „ „	Ages 15-45	14.4

It is thus completely demonstrated that all the statements by which the public has been gulled for so many years as to the almost complete immunity of the revaccinated Army and Navy are absolutely false. It is all what the Americans call 'bluff.' There is *no* immunity. They have *no* protection. When exposed to infection, they *do* suffer just as much as other populations, or even more. In the whole of the nineteen years 1878-1896, inclusive, unvaccinated Leicester had so few smallpox deaths that the Registrar-General represents the average by the decimal 0.01 per thousand population, equal to ten per million, while for the twelve years 1878-1889 there was less than one death per annum! Here we have *real* immunity, *real* protection; and it is obtained by attending to sanitation and isolation, coupled with the almost total neglect of vaccination. Neither Army nor Navy can show any such results as this."

So we find the efficacy of "that great fact of vaccination," which Pasteur took as the foundation of his medical theories and practice, described as "bluff" by the great scientist who stands alongside of Darwin in regard to the theory, correct or false, of Evolution. Not that it is his name that impresses us, but the testimony he puts forward, the verdict of time, the judgment of history. And the lessons of the past continue up to the present in Leicester, where for the 26 years ending 1931 there have been only two deaths from smallpox.

In the same way, the experience of Germany and of Japan shows us that with much vaccination there is also

much smallpox, while, perhaps, the Philippine Islands provide us with the most striking object lesson on record.

Since the taking over of the islands by the United States of America, every attention has been paid to the perfecting of sanitation. But not content with this, their Public Health Service has seen to the thorough systematic vaccination of the population, adding thereto a considerable amount of serum-inoculation. For the result, let us turn to an American paper, published in Minneapolis, *The Masonic Observer*, of the 14th January, 1922:—

"The Philippines have experienced three smallpox epidemics since the United States first took over the Islands, the first in 1905-1906, the second in 1907-1908, and the third and worst of all, the recent epidemic of 1918-1919. Before 1905 (with no systematic general vaccination) the case-mortality was about 10 per cent. In the 1905-1906 epidemic, with vaccination well started, the case-mortality ranged from 25 to 50 per cent in different parts of the Islands. During the epidemic of 1918-1919, with the Philippines, supposedly, almost universally immunized against smallpox by vaccination, the case-mortality averaged over 65 per cent. These figures can be verified by reference to the Report of the Philippine Health Service for 1919, see page 78. These figures are accompanied by the statement that 'THE MORTALITY IS HARDLY EXPLAINABLE.' To anyone but a Philippine Medical Health Commissioner it is plainly the result of vaccination.

"Not only has smallpox become more deadly in the Philippines, but, in addition, 'The statistics of the Philippine Health Service show that there has been a steady increase in recent years in the number of preventable diseases, especially typhoid, malaria and tuberculosis.' (Quoted from the 1921 Report of the Special Mission on Investigation to the Philippine Islands, of which Commission General Leonard Wood was the head.)"

Going more into detail in an earlier issue, the 10th December, 1921, *The Masonic Observer* writes:—

"The highest percentage of mortality, 65.3 per cent, was in Manila, the most thoroughly vaccinated place in the Islands, the lowest percentage of mortality, 11.4 per cent, was in Mindanao where, owing to religious prejudices of the inhabitants, vaccination had not been practised as much as in most other parts of the Islands. To the everlasting shame of the misnamed 'Health' Service, vaccination has been largely forced on Mindanao since 1918 in the face of this direct proof that their people were safer without it, and with the result of a smallpox mortality increase to above 25 per cent in 1920. In view of the fact that Sanitary Engineers had probably done more in Manila to clean up the city and make it healthy than in any other part of the islands, there is every reason to believe that excessive vaccination actually brought on the smallpox epidemic in spite of the sanitary measures taken to promote health."

Again from the issue of the 17th December, 1921, we may quote:—"Think of it—less than 11,000,000 population in the Philippines and 107,981 cases of smallpox with the awful toll of 59,741 deaths in 1918 and 1919, and bear in mind that, in all human probability, the inhabitants of the Philippines are as thoroughly vaccinated and re-vaccinated as any people in the world.

"Systematic vaccination started in the Philippines in 1905 and has continued ever since. It is certain that over Ten Million vaccinations for smallpox were performed in the Philippines from 1905 to 1917, inclusive, and very probable that the vaccinations numbered even as many as Fifteen Million during that time. This can be verified by reference to reports of the Philippine Health Service."

Turning to those Reports we find evidence that the facts must have been even worse. In his letters of transmittal to the Secretary of Public Instruction, Dr. V. de Jesus, Director of Health, states that in 1918 and 1919 there were in the Philippines, 112,549 cases of smallpox with 60,855 deaths. The Chief of the Division of Sanitation in the Provinces gives yet higher figures for the year 1919,

bringing the total for the two years actually up to 145,317 cases and 63,434 deaths.

So the verdict of Time pronounces against Jenner and Pasteur.

Yet, basing his theories upon a practice already discredited by those who had given it close impartial scientific study, Pasteur determined to inaugurate a system of preventive medicine to safeguard against what he proclaimed to be the ravages of air-borne microbes. The attenuated doses, which, according to his theory, were to be preventive of natural diseases, did due honour to Edward Jenner by being called vaccines.

Pasteur's son-in-law tells us[1]:—"Midst his researches on a vaccine for chicken-cholera, the etiology of splenic fever was unceasingly preoccupying Pasteur."

Although a vaccine for the former complaint was the first he professed to discover, it was in regard to the latter that a great stir was occasioned, for Pasteur was called upon in various instances to test his method of vaccination. We will, therefore, leave to the next chapter a study of his methods against anthrax, which form the starting-point of that subsequent fashion for inoculation which has proved so financially profitable to the manufacturers of vaccines and sera and has so disastrously clogged the calm dispassionate advance of science with the pecuniary considerations of commercial interests.

[1] *The Life of Pasteur*, p. 303.

CHAPTER XVI

The International Medical Congress and Some Pasteurian Fiascos.

It was in the year 1877 that Pasteur took up the subject of anthrax, and, as usual, pushing himself to the front, he advertised far and wide his method of cultivating the rod-like organisms, the bacteridia. These he claimed to have proved to be the sole cause of the complaint, which he proposed impressively to rename the disease of the bacteridia.

He asserted that the blood of an anthracised animal contains no other organisms but the bacteridia, which he considered to be exclusively ærobic. He argued that they, therefore, take no part in putrefaction which, according to him, is always due to anærobic micro-organisms of the order of vibrios, and that consequently anthracised blood of itself is imputrescible. In the corpse, on the contrary, he believed that anthracised blood quickly becomes putrescent, since, according to him, every corpse provides a home for vibrios which enter from without into the intestinal canal, always full of vibrios of all kinds, and so soon as the normal life does not hinder them they bring about a prompt disintegration.

This was the teaching upon which Pasteur was to build up his prophylaxis against anthrax, and so, for his prophylactic, he put forward a mixture of "ærobic germs," namely, the bacteridia, with "anærobic germs" of putrefaction. He maintained that a result would be obtained that should neutralise the virulence of the *bacillus anthracis*, and thus if injected into animals would protect them from infection.

It was while Pasteur was putting forward such views that he fell foul of another Member of the Academy of

Medicine, Dr. Colin, who asked how anthrax could be due to the bacteridia, when it was sometimes found in a virulent stage and yet devoid of the presence of these micro-organisms. He claimed the floor on the 12th March, 1878, to criticise the printed Report of the former Session.[1]

"M. Pasteur at the previous Session," he said, "had formulated two propositions, which are not to be found in the *Bulletin*. The first is that the bacteridia of anthrax do not develop in the blood of healthy animals; the second that the bacteridia will not supply germs to the organism. I replied that these two allegations seemed to me open to dispute, but all criticism of them becomes pointless, owing to their suppression from the printed record. Other statements of M. Pasteur have also been suppressed from the record, as printed, and among others the one that 'It would take a man his life-time to examine a drop of anthracised blood,' and also that 'The search for a bacteridium in a drop of blood is as difficult as that for a cell of a ferment in a litre of beer-yeast.'

"These suppressions, and some additions of which I need not speak, are absolutely a matter of indifference to me, although they make me appear as having spoken 'in the air' and without object. But what is not indifferent to me is that M. Pasteur represents me in the *Bulletin* as saying something I did not say, inserting as mine a mode of experimentation and of reasoning that are not mine at all. It is against this that I protest."

Pasteur gave a confused reply, which did not answer Dr. Colin's accusation, which, be it noted, did not concern the natural correction by an author of the report of his observations, but a direct juggling with the records. In the absence of any proper explanation and apology from M. Pasteur, we can quite understand Dr. Colin saying:[2]—"I declare that henceforth I will have no discussion with M. Pasteur."

[1] *Bull. de l'Académie de Médecine, 2e Vol.* 7, pp. 220-235.
[2] *ibid.* p. 261.

The glowing panegyrics that surround the memory of the famous French chemist considerably obscure the disfavour in which his methods were held by many of his contemporaries.

Pasteur lost no time in pushing his views on anthrax and kindred subjects, and on the 30th April, 1878, read before the Academy of Science a Memoir bearing his own name and those of Messrs. Joubert and Chamberland. It was entitled "The Theory of Germs and their Application to Medicine and Surgery," and was the first lusty trumpet-blast of the germ-theory of disease. Pasteur seized this good opportunity to advertise widely that he had discovered "the fact that ferments are living beings." It goes without saying that not one word of acknowledgment was made to Béchamp, for his wonderful illumination of the subject. The Memoir began by asserting that this discovery was a result of Pasteur's Communication in 1857-1858 upon fermentation; that the germs of micro-organisms abound everywhere; that the theory of spontaneous generation was thus shown to be a chimera, and that wine, beer, vinegar, blood, urine and all the liquids of the body undergo none of their ordinary changes in contact with pure air.

We have already seen, firstly, that in regard to fermentation in general and vinous fermentation in particular, as also in regard to silk-worm diseases, it is impossible to deny that Pasteur plagiarised Béchamp. Secondly, we have seen that Pasteur's experiments were insufficient to defeat the theory of spontaneous generation and that they never satisfied Sponteparists, such as Pouchet, Le Bon and Bastian. Béchamp's experiments and explanations alone seem to account for phenomena that without them can only be explained by heterogenesis. Thirdly, notwithstanding the assertions of this Memoir of triple authorship, both the liquids and solids of animal and vegetable bodies do undergo changes, by reason, so Béchamp explained, of the infinitesimal living organisms they contain, to which he gave the illuminating name of microzyma. Even

M. Pasteur hinted at belief in this when he claimed that "every being, every organ, every cell that lives or continues its life without the help of the oxygen of the air . . . must possess the character of a ferment." His own self-styled "famous experiment" on meat actually bore witness to such changes, although he denied them.

The authors of the Memoir went on to describe how, in their judgment, an infinitesimal quantity of their last produced culture was capable of producing anthrax with all its symptoms. On sowing their septic product (vibrios obtained from the carcass of an animal that had died of septicæmia), the authors found that their first efforts failed. Their cultures were not barren, but the organisms obtained were not the septic vibrios, but had the common form of chaplets of small spherical grains exceedingly minute and not virulent.

Similar observations had already been made by Professor Béchamp, who, with his collaborators, had demonstrated the connection between a disturbed state of body and the disturbed state of its indwelling particles, which, upon an unfavourable alteration in their surroundings, are hampered in their normal multiplication as healthy microzymas and are consequently prone to develop into organisms of varied shape, known as bacteria. Upon an improvement in their environment, the bacteria, according to Béchamp's view, by a form of devolution, may return to their microzymian state, but much smaller and more numerous than they were originally.

It is regrettable that expositions by Béchamp should have been set aside, especially as Pasteur and his friends could only account for the phenomena described in the Memoir by concluding that they had sown an unobserved impurity at the same time as the septic vibrio. They also put forward the contention that each micro-organism of a particular form and shape was a provocative disease-agent. Thus, according to them, the septic vibrio produced septicæmia, and the rod-shaped bacterium, usually associated with anthrax and since known as the *bacillus*

anthracis, was the direct originator of that torment of animals. They made, in addition, the dogmatic claim that their so-called proof was not open to dispute, although in their theory confusion reigned until the German, Dr. Robert Koch, came to their rescue and formulated a set of rules for the recognition of supposed disease-germs. According to him, these must be:—

1. Found in every case of the disease.
2. Never found apart from the disease.
3. Capable of culture outside the body.
4. Capable of producing by injection the same disease as that undergone by the body from which they were taken.

Here we see the basic theory of the air-borne disease-germ doctrine contradicted by the last postulate, for if to invoke disease, organisms require to be *taken from bodies*, either directly or else intermediately through cultures, what evidence is adduced of the responsibility of invaders *from the atmosphere?* As Béchamp showed[1]:—"In all the experiments of recent years it has been the microzyma proper to an animal and not a germ of the air that has been found to be the seat of the virulence. No one has ever been able to produce with germs obtained from the atmosphere any of the so-called parasitic diseases. Whenever by inoculation a typical known malady has been reproduced, it has been necessary to go and take the pretended parasite from a sick animal; thus to inoculate tuberculosis, the tubercle has been taken from a subject already affected."

It is noteworthy that neither Pasteur nor any of his successors have ever induced a complaint by the inoculation of air-carried bacteria, but only by injections from bodily sources. Furthermore, the verdict of time is pronouncing upon the microbian rules very fatally, and even medical orthodoxy has reluctantly to acknowledge[2] that "Koch's postulates are rarely, if ever, complied with."

But Pasteur, as we have seen, had all through his life little interest in speculative theories, so all-engrossing in

[1] *Les Microzymas*, p. 819. [2] *The Lancet* (March 29, 1909).

themselves to a devotee of Nature, like Béchamp; that is to say, Pasteur's mind always turned to the business side of any proposition. He now saw ahead a chance of tangible profit and dreamed of a means of arresting, or professing to arrest, the ravages of anthrax among sheep and cattle. Using his classification of ærobic and anærobic micro-organisms, he proposed by a mixture of the two sorts to neutralize the virulence of the bacteridium. We have already seen how he regarded the accidental administration of some stale culture to hens as a guide to his subsequent proceedings, and it was for chicken-cholera that he first endeavoured to procure what he called a "vaccine." Professor Toussaint, of the Toulouse Veterinary School, worked at the subject of "vaccination" against anthrax, which Pasteur subsequently took up and announced himself satisfied that he had discovered a real preventive.

In May, 1881, Pasteur was invited to put his vaccine to the test at a farm near Melun, and in June he wrote home triumphantly that complete success had resulted. By this was meant that sheep that had been first inoculated with his preparation did not succumb to a subsequent dosage of poison. The test was artificial. No real success could be proved unless it was found that natural infection was powerless against inoculated animals. This objection was put forward and in July some experiments were undertaken that were supposed to satisfy it, since the power of the vaccine was tested by a subsequent injection of blood taken from a sheep that had actually died of anthrax. But here again it is obvious that the procedure was distinct from natural infection, especially as certain sheep remained impervious to the complaint although feeding on ground supposed to be pervaded by bacteria from the buried carcasses of diseased sheep. However, success seemed sufficient for a commercial asset to be made of the supposed prophylactic. It does not take much observation to note that pecuniary profits obstruct unbiased criticism, and thus real investigation was checked from the first by Pasteur's alliance of science with commercialism.

In the midst of his experiments a break came. An International Medical Congress took place in London in August, 1881, and the French Republic sent Pasteur as its representative.

His son-in-law tells us[1] of the outburst of cheering that arose as he approached the platform after entering St. James' Hall; while quietly seated in his place amidst the great assembly, unnoticed, for the most part, sat the real discoverer of the fermentative rôle of micro-organisms of the air and of the internal tissues, the real elucidator of the mysteries of silk-worm diseases and vinous fermentation, the founder of views considered to be new even to-day by cytologists. Béchamp watched the triumph of his rival in silence. In a foreign assembly he would have been the last to cast any stigma upon a compatriot, and it never entered his head that Pasteur would go out of his way to attack him in the presence of strangers. But, unhappily, ambition often oversteps delicacy.

The incident took place at a Sectional meeting at which Professor Bastian put forward his view of the development of micro-organisms in internal tissues, his opinion differing from Béchamp's in that, instead of acknowledging living granulations, the microzymas, as parent units, it involved the spontaneous generation of organic from inorganic matter.

Pasteur, called upon to answer, went off at a tangent on the subject, and to refute Bastian, suggested a cruel experiment which in itself contradicts his apologists' attempts to whitewash his callousness towards animal suffering. *The Times* of the 8th August, 1881, quotes his words as follows:—

"If Dr. Bastian took the limb of a living animal, healthy or ill, provided the illness was not *microbienne*, bruised the tissues of it and reduced it to a most unhealthy condition, without, however, breaking the skin, and taking care to exclude microbes from the intestinal canal, he would never find in it the smallest microscopic organisms. Had Dr.

[1] *The Life of Pasteur*, by René Vallery-Radot, p. 329.

Bastian forgotten his (Pasteur's) experiment of 1863 by which he had shown that the blood and urine of a living animal introduced into glass vases could not putrefy, although exposed to free contact with the air, and with air, moreover, which was constantly renewed, provided only the air was free of germs? . . . In the study of microscopic organisms there was an ever present source of error in the introduction of foreign germs, in spite of the precautions that might be taken against them. When the observer saw first one organism and afterwards a different one, he was prone to conclude that the first organisms had undergone a change. Yet this might be a pure illusion. . . . The transformation of a *bacillus anthracis* into a *micrococcus* did not exist."

Alas! for Pasteur and the verdict of time upon a scientist! That same newspaper, *The Times*, which quoted his glib assertion, many years later, on the 8th April, 1914, thus wrote of the contradictory testimony of a worker at the very Pasteur Institute:

"Mme. Henri's discovery marks a step in the evolution of the science of bacteriology. Briefly stated, what has been accomplished is the transformation of a well-known bacillus of definite shape and possessing definite toxic properties into another type of micro-organism apparently possessed of properties of a kind entirely different from those of the original anthrax bacillus."

Or, as *The Daily News* of the same date put it:—

"The experiment was made with the anthrax bacillus, which from a rod shape was transformed into a spherical coccus."

So much for Pasteur's assertion that—"the transformation of a *bacillus anthracis* into a *micrococcus* did not exist." Though as to the newness of "Mme. Henri's discovery," Professor Béchamp could have explained it at the Medical Congress in the year 1881, when he was already familiar with the transformation of bacilli, both as regards form and function.

"This discovery (Mme. Henri's)," says *The Times*, "is

regarded as important and possibly marking a step towards finding some protoplasmic form of the origin of life."

This form would appear to be the minute granulations of cells, of which Professor Minchin was to treat a year later before the British Association for the Advancement of Science, and which had already been investigated by Béchamp since the sixties of the nineteenth century. We can imagine the trial it was to him to listen to assertions made by Pasteur upon matters that he could so easily have refuted. But, as he tells us in his Preface to *Les Microzymas*,[1] "I let him talk, because I was to speak after him."

This was when Pasteur, most unfairly, suddenly included his compatriot in his strictures against Spontepar-ists, speaking as though Béchamp were a believer in heterogenesis, instead of the real destroyer of the belief in spontaneous generation through his microzymian explanation of the presence of micro-organisms within internal organs and tissues.

The Times thus quotes Pasteur:—

"The same error was made in this respect by Dr. Bastian in England and Professor Béchamp in France. The latter was wholly mistaken, for instance, in his theory as to the existence of microzymas in chalk."

The Times, kind to the fashionable demagogue, leaves Pasteur's criticism at this; but what fired Béchamp's indignation was, as he tells us in his Preface to *Les Microzymas*, Pasteur's subsequent unpardonable accusation of plagiarism:—

"If there was anything exact in Béchamp's view-point, he had conceived it in assimilating his (Pasteur's) labours and modifying his ideas according to the other's."

Such a bare-faced reversal of facts was too much for long-suffering Professor Béchamp. He sprang from his seat and faced his traducer, indignantly demanding proofs and promising himself to supply them to establish the exact opposite.

[1] p. 7.

Pasteur's behaviour cannot, we think, be condoned by even his most enthusiastic admirer. Confronted by his victim, he simply turned on his heel and quitted the assembly, defrauding Béchamp of all opportunity for a proper public vindication of himself and his discoveries.

As *The Times* has quoted the latter's speech, we can see for ourselves the contrast of the Professor's magnanimous and dignified treatment of Pasteur.

"Professor Béchamp of Lille, likewise speaking in French, affirmed that the microzymas in chalk did exist and that if M. Pasteur had not obtained such results, it was because his experiments were badly conducted. On other points also M. Béchamp contested M. Pasteur's views. He held that the cause of disease and of death lay in the animal itself. The so-called 'molecular granulations' of histologists were living organised things, endowed with chemical activity, and having the same functions as the similar granulations which existed in the air and in chalk under the name of microzymas; they were the primitive agents of the organisation and the chemical activity of living organisms, though, strange to say, these microzymas, while morphologically identical, exercised different functions in different organic centres and tissues, as, for instance, the microzymas of the pancreas compared with those of the liver. He could not admit that they entered the tissues from the air. M. Pasteur denied their existence there because it conflicted with his theories. For his own part, however, he was convinced that tissues did show bacteria of different shapes and sizes where no penetration of germs from the air could have occurred. In M. Pasteur's experiment with blood and urine, these liquids really suffered a change and, so far from disproving the existence of microzymas in them, served to confirm it."

Pasteur was spared the difficulty of replying, since he had already withdrawn after his uncalled-for attack upon the fellow-countryman to whose researches he owed such a vast debt. Possibly it was this very fact that envenomed him against Béchamp. We are reminded of the story of the

man who, upon being told that a neighbour detested him, asked:—"Why should he? I have never done him a good turn."

Lionised by the big-wigs among whom he found himself, Pasteur felt secure in his triumph. At one of the great general meetings, at the request of the President, Sir James Paget, he gave a lecture upon his method of "vaccination" against chicken-cholera and anthrax, for which he naturally claimed unmitigated success, while he took the opportunity to extol Edward Jenner, relegating himself and his own works to what was, certainly, very suitable company. Delighting almost childishly in the flatteries that had been showered upon him, announcing his triumph in private letters, Pasteur returned to France, where a fresh honour soon overjoyed him, his election to the French Academy. He was growing so accustomed to riding down like a Car of Juggernaut any contradictions that dared to uplift themselves that it was very galling to him when, about this time, the wheels of his triumphal progress met with obstructions from abroad.

His biographer tells us[1]:—"The sharpest attacks came from Germany." Dr. Koch and others disputed Pasteur's conclusions and dared to doubt the efficacy of his prophylactic against anthrax.

At home, too, there were annoyances. At the Academy of Medicine voices were raised against the germ-theory of disease, and in particular M. Peter ridiculed the all-conquering microbe. It was the easier for him to do this, as in March, 1882, the boasted success of the vaccine for anthrax had met with a disastrous downfall.

It had come about in this way. In Italy it had been thought worth while for a Commission composed of Members of the University of Turin to perform experiments such as Pasteur had described and thus test his prophylactic. As a result, to quote M. René Vallery-Radot[2]:—"All the sheep, vaccinated and unvaccinated, had succumbed sub-

[1] *The Life of Pasteur*, by René Vallery-Radot, p. 357.
[2] *The Life of Pasteur*, pp. 367, 368.

sequently to the inoculation of the blood of a sheep that had died of *charbon*." No failure could have been more complete.

Pasteur wrote for particulars and was informed that the sheep which had been used for the experiment had died of anthrax on the 22nd March, 1882, and that the following day its blood had been inoculated into other sheep, every one of which had died as a consequence. According to Pasteur's theories, this should not have happened, for in a Communication on the subject to the Academy of Medicine on the 17th July, 1877, he had maintained that blood from the heart would not be virulent even though taken from an animal already putrid and virulent in many extensive parts of its body. Pasteur tried to wriggle out of the dilemma by denying that this applied to an animal that had been dead for twenty-four hours. He claimed that the catastrophe was due to a mistake on the part of the Turin Professors who had inoculated blood that had been septic as well as tainted by anthrax.

The eminent Italians, men of excellent standing, were naturally very indignant at his accusation that they did not know how to recognize septicæmia and that a man, by the way, neither a doctor nor a veterinary surgeon, should consider himself able from Paris to diagnose conditions in an animal on which he had never set eyes.

For a year a battle royal waged hotly between the Turin Veterinary School and M. Pasteur, who, finally, in the Spring of 1883, wrote and offered to go to Turin and personally repeat the experiment in which the Professors had failed so signally and show that the blood of an anthracised carcass would be also septic on the second day after death. But M. Pasteur was now dealing with men of the race of Machiavelli. These Italians at once saw how easy it would be to make such an experiment appear to succeed by some trickery. They were determined to safeguard its repetition under exactly similar conditions to their own disastrous trial. They, therefore, replied to Pasteur that, as a condition of the acceptance of his offer, he should first give some

precision to his proposed experiments by informing them:—

1. What, in his opinion, would be the microscopic characters presented by the blood of a sheep, taken directly from the heart, when it is at the same time septic and anthracised?

2. What, in his opinion, would be the genus and course of disease, and what would be the macroscopic and microscopic changes that should be expected to be found in sheep and in horned cattle made ill and even killed by the inoculation of this blood? Such experiments, in the opinion of the Professors, would be necessary to complete those proposed by Pasteur.

The astute Frenchman had now no simple innocents to deal with. He was requested to set down in black and white definite descriptive statements, which would be faced by hard facts and run the grave risk of being found wanting. This reasonable test of his views, which any scientist should have welcomed, was to him a trap, into which he had no intention of walking. The way of escape lay in throwing the onus on the Italians, and in a Communication to the Academy of Science,[1] he actually dared to say[2]:—"The Commission of Turin then does not accept my offer to go to them!" He was careful to keep from the Academy the letter he had received in which his suggestion was by no means declined, but merely made accessory to preliminary clear statements in regard to the proposed experimentation. What Pasteur, however, did not hesitate to do was to accuse the Commission of erroneous statements and quotations. His biographer is careful to avoid telling us that he was promptly challenged to point these out. He did so by quoting an extract the Commission had taken from his own statement of the 17th July, 1877, that in which he had said:—"The blood from the heart will not be at all virulent, although it be taken from an animal already putrid and virulent, in several extensive

[1] *Comptes Rendus* 96, p. 1457.
[2] *ibid.* p. 1459.

parts of the body." To this he made retort: "I have never written anything of the sort with regard to an animal that has been dead 24 hours." He went on to quote his own version of what he had said, winding up:—"The blood will not be at all virulent, although it be taken from an animal already putrid in several parts of its body." The Commissioners, having the text of his Communication of 1877 before them, were able to reply that Pasteur, even when quoting himself, had omitted the words "and virulent" after "putrid" and "extensive" before "parts," thus manipulating his own statement.

They published this communication of Pasteur's together with their own criticism in a pamphlet entitled *Of the Scientific Dogmatism of the Illustrious Professor Pasteur*, which was issued on the 10th June and translated into French in August, 1883, and bore the signatures of Vallada, Bassi, Brusasco, Longo, Demarchi, and Venuta, all men of high character and reputation.

In this document it was pointed out that Pasteur seemed to have forgotten that the putrid decomposition of bodies might vary in rapidity according to the temperature of March, a month notably changeable in its climatic relation to time and place. The Professors now explained that they had regarded Pasteur's offer as a trick and that, not being the fools he had taken them for, they had decided that they must know what he understood by the term "septicæmia," and that the experiments should be made fully and under the conditions and in the way that they had followed in their own experiment of March, 1882. With cutting irony, the Commission rejoiced with their illustrious opponent for having at last admitted that the inoculation of blood at once anthracised and septic could, according to the relations of the two taints in the blood doubly infected, produce sometimes pure anthrax, sometimes pure septicæmia, and sometimes anthrax and septicæmia combined. By this admission he destroyed his own dogma of the non-development of the bacillus of anthrax when it is associated with other organisms, ærobic or an-

ærobic. The Commission further congratulated themselves on having convinced M. Pasteur that he could not at Paris diagnose the complaint of an animal that had died at Turin, and they were glad that they had led to his reviewing his dogmas through the researches of his assistant, M. Roux, and recognising as erroneous the following principle laid down in his Communication of July, 1877:—"The bacteria of anthrax may be profusely introduced into an animal without giving it anthrax. It will be sufficient if the bacteridia suspended in the liquid have at the same time the common bacteria associated with them."

The Commission pointed out that Pasteur's assertion that the blood of an anthracised carcass would be septic after 24 hours was as much as to describe septicæmia as a necessary consequence of the progress of putrefaction, reasoning they considered narrow and inconsistent with facts. They compared various statements of Pasteur's taken from his Communication of July, 1877, and from his Memoir of 1878 on "The Theory of Germs and their Application to Medicine and Surgery."

He had stated:—"The blood of an anthracised animal contains no other organisms than the bacteridia, but the bacteridia are exclusively ærobic. They therefore take no part in the putrefaction; thus the anthracised blood is not capable of putrefaction by itself. But in the carcass, things happen differently. The anthracised blood enters rapidly into putrefaction, because all corpses give a home to vibrios coming from without, that is to say, in the present case, from the intestinal canal, which is always filled up with all kinds of vibrios.

"The septic vibrio is none other than one of the vibrios of putrefaction."

After asking himself whether septicæmia or putrefaction in a living subject is a special disease, he answers:—"No! So many vibrios, so many different septicæmias, benign or malignant."

Yet in his Memoir on the Germ-Theory, he asserts:—"We have only met one single vibrio in septicæmia,

properly so-called, which the media in which they are cultivated cause to change in appearance, as regards facility of propagation and of virulence."

After many other quotations, the Commission summed up that it was clearly to be deduced that, according to the illustrious M. Pasteur, the blood of anthracised carcasses must be necessarily and fatally septic in 24 hours or less, because it contains the vibrios of putrefaction. They sarcastically referred to his admission of septicæmias benign and malignant, "but it seems," they said, "that the vibrios of the benign septicæmias reside in Paris only and that in Italy they do not exist, because he has declared positively that the unfortunate animals which died as a result of our former experiment on the 23rd March, were killed by septicæmia, which having succeeded in killing must, without doubt, belong to the category of the malignant. Notwithstanding the competence of the illustrious M. Pasteur in such an argument, we venture to differ from him, and, to show that our opinion is correct, we will say in a few words that some of our experiments have proved that even in Turin there are vibrios of benign septicæmia, that is to say of septicæmia that is not fatal; and they have further proved that the blood of sheep and horned cattle suffering from anthrax, the blood of the latter not anthracised, the juice of the flesh a prey to putrefaction, containing septic vibrios in the sense understood by the illustrious M. Pasteur, may sometimes produce neither pure anthrax nor pure septicæmia, nor anthrax and septicæmia combined. . . . And that such negative result may be obtained even when the blood contains millions of the vibrios that the illustrious M. Pasteur regards as septic, and when these are in very active movement."

The pamphlet then describes the Commission's experiment in fullest detail, showing how lowered conditions of temperature, etc., must have retarded putrefaction and that, according to Pasteur's own dogmas, it was "certain that there were neither vibrios of putrefaction nor other evidence of septicæmia in the blood inoculated into our

animals, vaccinated or non-vaccinated. But suppose that there had been the vibrios of septicæmia and that neither we nor other competent persons had perceived them, what then ought to have happened according to the dogmas proclaimed by the illustrious Pasteur in 1877? Either the little droplet or two, spread out in a thin layer upon the wound of each animal and exposed to the action of air, would become harmless as a septic agent of infection, because the vibrios, thronging the septic fluid in the form of moving threads, were destroyed and disappeared on contact with the air, since it was said that air seems to burn the vibrios. But, in this case the *bacillus anthracis* ought to be able to develop easily, as, being ærobic, it would not have to struggle, on contact with the air, with anærobic vibrios. Or else the vibrios are not destroyed on contact with the air . . . and in this second case there would necessarily develop in the inoculated animals a malady that by its course, its duration, its symptoms and its lesions would reveal characters proper to septicæmia and to septicæmia only. But in such case, lesions of septicæmia and not of anthrax should be found in the carcass. . . . Even admitted as a hypothesis that the blood of the anthracised sheep, which we employed on the 23rd March, had also been septic, but that we in our crass ignorance and incapacity were unable to perceive it, nevertheless, it could not have produced in the animal, inoculated in the way that we have described, anything but pure anthrax. This result, which, before the new experiments of M. Roux, was passionately contested by our illustrious opponent because he thought it improbable, is to-day admitted to be possible, because it does not find itself any more in contradiction with the new dogma, reformed in accordance with the new results of experiments of the month of May, 1883, which he has communicated to the Academy of Science in Paris."

The pamphlet winds up by showing that the quotations of the Commission had been accurately given, and that it was M. Pasteur who had suppressed certain words to

modify his original assertion. Moreover, although he had asked the Commission to correct the faults in the French translation of their Italian Report, he actually published this in the *Revue Scientifique* without paying the slightest attention to the all too numerous corrections of mistakes that put a totally different construction upon the original signification.

Perhaps it is scarcely to be wondered that while the Turin controversy was raging, his son-in-law should put on record[1] that Pasteur "was tired of incessant and barren struggles." The Italian Professors, however, did not consider their time to have been wasted. On the contrary they declared themselves satisfied, "because we have attained our proposed end, the research and demonstration of truth and the refutation of error."

It is only to be regretted that this attitude of scientific doubt should have given way to the simple credulity, the unquestioning faith, of modern medical orthodoxy towards almost any dogma enunciated by the followers of Pasteur.

It did not require much perspicacity to realise that if Pasteurian treatment could secure any appearance of success, the pecuniary advantages would be considerable. Thus Pasteur inaugurated the era that was to see the calamitous prostitution of science to commercialism. Bacteriological Institutes for experimentation upon living animals and for the production and sale of vaccines and sera came into being all over the world, modelled upon the one opened in 1888, in Paris.

Odessa was one of the places early provided with such an institution; but the history of its initiatory traffic in the anti-anthrax vaccine was calamitous.[2]

The nostrum was sent to Kachowka in Southern Russia, where it was administered, according to Pasteur's description, to 4,564 sheep, of which number 3,696 were very soon

[1] *Life of Pasteur*, p. 369.

[2] See introductory letter from Professor Peter (pp. 8 and 9) to *Études sur la Rage, par le Dr. Lutaud.*

dead. The first vaccinations were performed in August, 1888, by Dr. Bardach, commencing on the 8th. 1,582 mother sheep were divided into two flocks. One lot was vaccinated before 11 a.m. of which one sheep died within 24 hours and 7 others within 36 hours of the operation. The second lot was vaccinated on the evening of the 10th August. The first to die succumbed during the night of the 9th-10th August. The greatest mortality occurred on the 10th and 11th. Of the 1,582 sheep vaccinated, 1,075 died from the effects—a percentage of 68.

Another trial took place at a farm belonging to a man called Spendrianow. The first flock consisted of 1,478 sheep of one, two and three years of age. The other flock consisted of 1,058 sheep, some older than those in the first lot and some younger. The sheep were vaccinated on the 10th August between 7 and 11 a.m. The next day at 1 p.m., the first death took place; the following day the mortality was at its highest and it diminished from August 13th. Altogether out of 4,564 animals vaccinated, as many as 3,696 died—a percentage of 81.

Thus the Turin disaster is shown to have been by no means an isolated example, and, in answer to Pasteur's supposed benefactions, these unfortunate animals, had they been given a voice in the matter, would certainly have prayed to be delivered from such a friend. Moreover, M. René Vallery-Radot, in his biography, tells us nothing of the private owners in France and elsewhere whom Pasteur had to compensate for animals killed by his vaccine.[1] A special Commission in Hungary recommended the Government of that country to prohibit its use; Koch and Müller in Germany pronounced against it; the English Board of Agriculture declined to recommend it; while, finally, before the last Royal Commission on Vivisection, its protagonists could not do better than damn the modern "modified" edition with faint praise.

Alas! for Pasteur and his pronouncement that "the only sovereign judge must be history!"

[1] *Études sur la Rage, par le Dr. Lutaud*, p. 419.

CHAPTER XVII

Hydrophobia

To the average man or woman of the present day, the mention of the name, Pasteur, immediately conjures up the thought of a horrible malady, hydrophobia. For to many, with the haziest notions of his connection with fermentation, silk-worm troubles and anti-anthrax inoculation, his fame is emblazoned on honour's roll as the saviour of humanity from the ravages of mad dogs!

The pity is that since Pasteur's day there should have been so much scare on the subject, for hydrophobia is a complaint of the nerves and, consequently, fear is its primary factor. Various instances have been recorded of cases unquestionably brought on by suggestion. For example, two young Frenchmen were bitten at Havre by the same dog in January, 1853. One died from the effects within a month, but before this the other young man had sailed for America, where he lived for 15 years in total ignorance of the end of his former companion. In September, 1868, he returned to France and heard of the tragedy, and actually then himself developed symptoms and within three weeks was dead of hydrophobia![1] Again, a patient who threatened to bite his medical attendant, after being told that the correct symptom in a human being was the use of the fists, struck out all round him like a boxer and indulged up to the time of his death in this quite novel form of paroxysms.[2]

The avoidance of fear is, therefore, the main essential of safety after a dog-bite, and the very slight amount of risk may be realised by the thousands of innocuous bites received by veterinary surgeons and others in the habit of constantly handling animals. Occasionally there may be a

[1] *Études sur la Rage, par le Dr. Lutaud*, p. 262.
[2] *Études sur la Rage, par le Dr. Lutaud*, p. 269.

victim to a bite in the same way that deaths have been known to occur after pin-pricks and stings of insects, while scratches and wounds sometimes bring about tetanus, of which complaint hydrophobia appears to be a variety.

According to Sir Victor Horsley's evidence before the Lords' Committee on Rabies,[1] the liability to hydrophobia after dog-bite among the untreated has been variously calculated to be from 5 to 15 per cent. A French authority, named Bouley, has stated that of 100 persons bitten by rabid animals and entirely untreated in any way whatever, not more than 5 would develop symptoms of hydrophobia.

Thus, happily, the victim of a supposed mad dog stands a very good chance of escaping any trouble. To begin with, it has to be remembered that there is considerable doubt of there being any such specific disease as rabies, and a "mad dog," in the popular sense, may possibly be relegated to the same category as the "witch" of the Middle Ages! The neglected lives of the pariah dogs of the East are sufficient to account for many finally suffering from the paroxysms and other symptoms that go by the name of rabies; and when we contemplate the chained existences of numbers of dogs in Europe our only wonder is that more do not develop madness. It may safely be said that a healthy happy life is the best safeguard against the trouble. For an animal to be in a savage state or to foam at the mouth is no real indication of rabies. For instance, in *A System of Surgery*,[2] we read:—"Some idea may be gained of the frequency of mistakes of diagnosis in connection with canine rabies by the statement of Faber, who says that, of 892 dogs brought into the Veterinary Institute of Vienna under suspicion of rabies, only 31 proved to be really affected." During a scare in England, according to the *Field* of April 19, 1919, Mr. Robert Vicary, a well-known kennel owner, believed that "many of the experts called in to diagnose the supposed cases of rabies were quite wrong in their reports." It seems likely

[1] *Minutes* 215.

[2] By T. Holmes, M.A. (Cantab)., and J. W. Hulke, F.R.S., p. 329 (note).

that many animals were merely suffering from a past scarcity due to war-time conditions; as wrong feeding has been known to produce symptoms like those of so-called rabies, as evidenced in the scare in the Klondyke, in 1896, an account of which has been given in the *Journal of Zoophily* by Arnold F. George.[1]

It is clear that more fear than intelligence is shown in regard to rabies, particularly as animals suspected of it are almost invariably put to death summarily instead of being kept alive *under kind and careful observation.* Moreover, once they are dead, the complaint cannot be traced by a *post-mortem* examination. The test applied is the one introduced by Pasteur, and this brings us to his commencement of work on the subject.

It was in the year 1880 that two mad dogs were presented to him for investigation by M. Bourrel, an Army Veterinary Surgeon. Then began the series of observations, very cruel for the most part, that resulted in the proud announcement to the Academy of Science at Paris of a process that would, so Pasteur maintained, infallibly prevent rabies from developing in persons who had undergone the misfortune to have been bitten by rabid animals.

The date of this Communication, the 26th October, 1885, was made by it "memorable in the history of medicine and glorious for French Science," according to the enthusiastic praise of the chairman, M. Bouley. The day was also memorable for the inauguration of a system of intolerance, the antithesis of all that is scientific, which has, unfortunately, continued in regard to the fetich-worship of Pasteurian orthodoxy. On this past eventful date, it was carried to the length of refusing to hear a word from M. Jules Guérin, Dr. Colin and others, who dared to venture criticism against the conclusions of M. Pasteur. The great man had spoken. He had dared to claim infallibility—"I call my method perfect." It behoved others either to praise or else to hold their peace.

Yet how much there was to criticise! The very inocula-

[1] See also article *Rabies and Hydrophobia* by L. Loat, in the *Bombay Humanitarian* for April, 1920.

tion test for proving madness was quite uncertain. This test, introduced by Pasteur, is to take some matter, the saliva, blood, part of the brain or spinal cord, usually the cerebro-spinal fluid, from the suspected animal and inject it into a living rabbit. It is evident to common sense, apart from Béchamp's illuminating explanation,[1] that matter from one creature introduced into another is likely to be injurious, and Vulpian, a French doctor and physiologist and a supporter of Pasteur, himself found that the saliva of healthy human beings killed off rabbits as quickly as the saliva of a child who had died of hydrophobia. The condition of a rabbit after inoculation proves nothing except the strength or weakness of its powers of resistance; and yet the paralysation of the hind quarters of a rabbit is made the test of rabies in the dog from which it received the injection. True, that nowadays rabid dogs are said to have *negri* bodies in the nerve-cells, or their branches, and these are claimed to be not causal, but diagnostic agents; but considering the contradictions and mistakes in regard to bacteria and disease, we may well question a diagnosis that depends upon these *negri* bodies, especially as it does not seem to have been proved that they are always absent in other diseases.

So much for the test, now as to the prophylactic, what changes Pasteur made from the start in his nostrum! In 1884, at a Medical Congress at Copenhagen, he announced that by weakening the virus from dogs (supposedly mad) by transmission through monkeys and by fortifying it again through rabbits, he had obtained something protective to dogs and which would eradicate rabies from the world. Considering that nothing then was, or now is, known of the cause of rabies, if regarded as a specific malady, as it was in Pasteur's opinion, surely such a boast savours very much of the "cure-alls" of quackery. Pasteur himself had to admit that he had not succeeded in rendering "refractory" more than 15 or 16 out of 20 dogs. Afterwards he abandoned the monkey as a transmission

[1] See *Les Microzymas*, p. 690; also p. 276 of this work.

agent, having originally chosen it, he said, because of its physical resemblance to man. In a pamphlet *Hydrophobia and Pasteur*, by Vincent Richards, F.R.C.S.,[1] the author pertinently asks:—" Does the result that 15 or 16 out of the 20 dogs inoculated remained unaffected in any way warrant the assumption that the method adopted by Pasteur was protective?"

On the 26th October, 1885, Pasteur described his later method of treatment, which was to take the spinal cords of rabbits that had received injections of virus, keep these for varying lengths of time, then beat them up, each with twice its own weight of sterilised bouillon, finally, commencing with the weakest, inoculate the patient for ten days successively. Moreover, he triumphantly pointed to a successful case, that of Joseph Meister, a little Alsatian boy, nine years old, who had been badly bitten by a dog on the preceding 4th July, 1885, and two days later was taken to Pasteur for treatment.

This being the crucial case upon which the famous Frenchman inaugurated his claim to success, it may be as well to review it.

The worst of the many severe bites received by the child were cauterised the same day with carbolic acid. At 8 p.m. on the 6th July, Pasteur, by means of a Pravaz syringe, inoculated the boy with some drops of his broth of spinal cords, taken from rabbits that had died of the paralytic complaint induced by injections into the brains of these poor little animals. For the succeeding ten days, Joseph Meister was regularly inoculated, receiving in all about a dozen injections of the spinal cord dosage.

Now, in considering this case, we must ask what proof Pasteur had of the madness of the dog and probability of hydrophobia ensuing in the victim?

The rabid state of the animal was inferred by its savagery and the fact that a *post-mortem* examination disclosed "hay, straw and pieces of wood"[2] in the stomach.

[1] Thacker, Spink and Co., Calcutta (1886).
[2] *The Life of Pasteur*, by René Vallery-Radot, p. 414.

The presence of the latter would seem far more likely to indicate that the dog had been ravenous, probably starving, a condition that, in itself, would have accounted for its savage behaviour. As to the boy, the number and severity of the bites he had received caused the doctors Vulpian and Grancher, who were called in, to decide that he was almost inevitably exposed to contract hydrophobia in consequence. Why? As we have seen, there was no real proof of rabies in the dog that had attacked him. But, for argument's sake, granting that the animal had been mad, it must be remembered that the wounds had been cauterised. Though opinions differ as regards cauterisation, many authorities seem strongly in favour, and reference may be made to Youatt's cauterisation of upwards of four hundred persons, including such application five times on himself, without hydrophobia developing in a single case.[1] Dr. Cunningham, of Chicago, reported as cauterising one hundred and twenty persons annually, has averaged the mortality as about three in that number. Pasteur himself once wrote to a doctor near Paris as follows:—"Sir:—The cauterisations that you have carried out ought to reassure you fully as to the consequences of the bite. Attempt no other treatment: it is useless. L. Pasteur."[2] Apart from cauterisation, the chance of hydrophobia developing in a person bitten even by a so-called genuinely mad dog has been seen to be small; and, moreover, as incubation has been known to extend to twelve months, often to two years, or more, the danger for Joseph Meister had obviously not been ended when, after little more than the lapse of three months, Pasteur dared to acclaim him as a brand snatched from the burning, so to speak, by his spinal cord dosage. Finally, other persons, including the dog's owner, Max Vone, bitten by the same dog as Meister and on the same day, who were neither cauterised nor treated by Pasteur, continued in good health. Thus we see that this first much vaunted case of

[1] Referred to in *Rabies and Hydrophobia*, by Thomas M. Dolan, L.R.C.P.

[2] *Études sur la Rage, par le Dr. Lutaud*, p. 23.

Pasteurian success has no more to be said for it, when examined carefully, than that Joseph Meister, so far as his history is known, does not appear to have come off better or worse through Pasteur's treatment than several others who went without it.

But all were not so fortunate as the little Alsatian. Another child, Mathieu Vidau, inoculated by Pasteur and supposed to be cured, died seven months after treatment.[1] To excuse the death of again another child, named Louise Pelletier, failure was attributed to the bites being on the head and too much time having elapsed after the bite before the inoculation: yet Pasteur claimed that his treatment would be successful if commenced at any time before hydrophobia set in, even after a year or more. Contradictions seem to have been of no account when needed as excuses, so much so that an American, Dr. Dulles of Philadelphia, has said that on placing Pasteur's statements side by side, the acceptance of almost any one demands the obliteration of the others!

The late Dr. Charles Bell Taylor, in the *National Review* for July, 1890, gave a list of cases in which patients of Pasteur's had died, while the dogs that had bitten them remained well.

A notable failure was that of a French postman, named Pierre Rascol, who, with another man, was attacked by a dog, supposed to be mad, but not bitten, for the dog's teeth did not penetrate his clothing; but his companion received severe bites. The latter refused to go to the Pasteur Institute and remained in perfect health; but unfortunate Rascol was forced by the postal authorities to undergo the treatment, which he did from the 9th to the 14th March. On the following 12th April, severe symptoms set in, with pain at the points of inoculation, not at the place of the bite, for the reason that he had never been bitten. On April 14th, he died of paralytic hydrophobia, the new disease brought into the world by Pasteur.[2]

[1] See *Études sur la Rage, par le Dr. Lutaud*, pp. 245, 246, and following.

[2] *Études sur la Rage, par le Dr. Lutaud*, p. 277-8. For a somewhat similar case regarding a Frenchman named Née, see the same work, p. 345.

What wonder that Professor Michel Peter complained: "M. Pasteur does not cure hydrophobia; he gives it!"

Certainly, it may be admitted that Pasteur never professed to have a cure. What he undertook was to prevent the development of a poison that he compared to a slow train, which, in the human system, was overtaken, according to him, by his protective express, the inoculated virus.

Already, in his own day, there were many unbelievers in his method. To these, in the London *Lancet* for May 15, 1886, the following caution was addressed by Dr. G. H. Brandt, evidently a sincere believer in the words and works of the famous French chemist:—"To the unbelievers M. Pasteur says:—Wait! Time will reveal many facts connected with this question, and it is only by continual experience and constant observations carried on for a considerable time on hundreds of cases that we shall be able to arrive at positive and definite results."

Many years have gone by since these words were penned, and we find ourselves now in a position to study the experience and observations for which earlier critics were told to be patient.

The claim for Pasteur's success is based upon the assertion that he reduced the death-rate for hydrophobia from 16 per cent to 1 per cent. But the late Colonel Tillard has shown in a pamphlet[1] called *Pasteur and Rabies*, that the 16 per cent theory of death-rate before Pasteur brought in his supposed preventive must be ridiculously wrong. As the yearly average number of deaths for France up to then had not been more than 30, the number of the bitten, according to the 16 per cent estimate, says Colonel Tillard, should have been less than 200; but Pasteur, on the contrary, had 1,778[2] patients during the year 1887, which meant, according to this calculation, that over 250 would have died had they not gone to him.

[1] Published by the British Union for the Abolition of Vivisection, 47, Whitehall, London, S.W.1.

[2] This is the number given in the article on Hydrophobia in Allbutt's *System of Medicine* by Prof. G. Sims Woodhead, M.D.

This is nothing short of an absurdity in view of the facts, the highest total of deaths ever recorded for any year having been 66!

More than this, if we turn from France to other countries, we find that at Zürich, for instance, of 233 persons bitten by rabid animals in a period of 42 years, "only 4 died, 2 of whom were bitten in parts where preventive measures could not be adopted."[1] Again, "Wendt of Breslau treated 106 persons bitten by mad animals between the years 1810 and 1823. Out of this number 2 died."[2] Once more, during an epidemic of rabies in Stockholm in 1824, 106 bitten persons presented themselves at the Royal Hospital, only 1 of whom contracted hydrophobia.[3] Many more instances might be enumerated, such, for example, as the gunpowder treatment formerly carried out in the Island of Hayti, where, though dog-bites were common, hydrophobia was practically unknown.[4]

Such results of pre-Pasteurian treatment surpass the best boasts of Pasteur and upset the truth of the 16 to 1 per cent reduction in mortality. Even were the latter claim correct, it would merely be brought about by the huge multiplication of cases, a method of jugglery continually found in statistics, and which, as Dr. Boucher of Paris, points out[5], does not prevent deaths from hydrophobia increasing, while the percentage decreases!

As to this increase, facts speak only too painfully. Before Pasteur's treatment, the average number of deaths *per annum* from hydrophobia in France was 30: after his treatment, the yearly average number increased to 45. The late Professor Carlo Ruata gave the annual average mortality from hydrophobia in Italy as 65 before the Pasteur treatment, and complained of its increase to 85

[1] *Rabies and Hydrophobia*, by Thomas M. Dolan, L.R.C.P., etc., p. 155.
[2] *ibid.* pp. 155-156.
[3] *ibid.* p. 156.
[4] *ibid.* pp. 188-9.
[5] *Anti-Rabic Inoculations: Their Deadly Effects*, by Dr. H. Boucher, published by The Animal Defence and Anti-Vivisection Society, 35, Old Bond St., London, W.1.

after the installation of 9 anti-rabic Institutes. We cannot, therefore, wonder at the criticism that he published in the *Corriere della Sera*:—"The numerous 'cures' that are boasted of in our nine anti-rabic Institutions (in Italy) are *cures* of bitten persons in whom the rabies would never have developed, even if they had not been subjected to the anti-rabic inoculations; and the small number of failures represent precisely the number of those in whom the rabies has taken, and who, for that reason, die after the inoculation, as they would have died without it. This is the mildest judgment that can be passed on the work of our nine anti-rabic Institutes, even if we might not unreasonably ask if some of the inoculated persons were not killed by the inoculations themselves."

As a comment on this, we can add that the National Anti-Vivisection Society has collected a list of 1,220 deaths after Pasteurian treatment between 1885 and 1901, and that the British Union for the Abolition of Vivisection is making a further list, which amounts already to nearly 2,000, and that every one of these deaths after treatment has been taken from the official returns of Pasteur Institutes.

In regard to the statistical returns of these Institutes, we will quote Dr. George Wilson's summary in his *Reservation Memorandum of the Royal Commission on Vivisection*:—"Pasteur carefully screened his statistics, after some untoward deaths had occurred during treatment or immediately after, by ruling that all deaths should be excluded from the statistical returns which occurred either during treatment or within fifteen days of the last injection. . . . It is in accordance with this most extraordinary rule that the percentage of deaths, in all Pasteur Institutes, works out at such a low figure. Thus, in the Report on the Kasauli Institute for 1910, Major Harvey commences his comments on the statistics of the year as follows:—'In this year, 2,073 persons, bitten or licked by rabid or *suspected* rabid animals, were treated'—yielding a percentage of failures of 0.19. This percentage Major

Harvey explains in these words:—'There were twenty-six deaths from hydrophobia. Of these fourteen died during the treatment, eight within fifteen days of completion of treatment, and four later than fifteen days after completion of treatment. Only the last four are accounted as failures of the treatment according to the usual definition of a failure, and it is on this number that the percentage failure-rate is calculated'."

This screening of statistics prevents the inclusion of the death of the late King Alexander of Greece among the list of Pasteurian failures. The announcement was made, after a monkey had bitten the King, that expert advice had been summoned from Paris. Had the King lived, no doubt a pæan of victory would have proclaimed his rescue through Pasteurian methods. As the King instead, unhappily, grew rapidly worse, a discreet silence was, for the most part, observed as to his treatment, the truth as to which, however, we learn in a bulletin received by the Greek Legation in London and reported in the *Daily Mail:*[1]—"Athens. Saturday. The King passed a critical night. His fever attained 105.6 deg. Fahr. and was preceded by severe shivering and accompanied by a fit of delirium, which lasted one hour and a half. This morning *he was again vaccinated.* His heart has weakened. His breathing is irregular." As the King thus died during the course of treatment, we must not only blame the monkey and not the vaccination for his death, but must not even count the latter as a failure of Pasteurian treatment.

Another more recent case cannot be thus excluded from this category. The *Daily Mail* of January 14, 1921, reports:—"A rare case of hydrophobia was revealed in Paris yesterday when Mme Gisseler, a Dutch woman, died as the result of having been bitten by a mad dog eight months ago. After the bite Mme Gisseler was immediately treated at the Pasteur Institute and altogether received 25 injections of serum." The excuse then follows that:—"such cases of death after treatment are extremely

[1] 18th October, 1920.

rare"; which announcement loses its force when we consider the many deaths, like that of the late King of Greece, excluded by an arbitrary time-limit from the table of failures.

Apart from the so-called "accidents" of treatment and apart also from deaths after treatment, from whatever cause, an additional argument against Pasteur's method is its introduction of a new disease, paralytic hydrophobia, entirely different from the many forms of pseudo-rabies. That this complaint is often wrongly attributed to other causes, "syphilis, alcoholism, or even influenza," and in other cases slurred over altogether, is disclosed in a Report, entitled *Paralysis of Anti-Rabies Treatment*, by Dr. P. Remlinger, Director of the Pasteur Institute, Morocco, to the International Rabies Conference held at the Pasteur Institute, Paris, from April 25 to 29, 1927.[1]

"We were impressed," he writes (p. 70), "with the discrepancy between the number of observations published by directors of institutes and the number of cases orally acknowledged by them to have occurred. Such occurrences were commonly kept secret, as if they were a reflection on the Pasteur method or a reflection on the doctor who applied it. Such a policy appeared to us to be clumsy and the reverse of scientific." And again (p. 85) "We have come to the conclusion that certain institutes conceal their cases. On various occasions we have found in medical literature observations concerning paralysis of treatment, and we have afterwards failed to find in the report and statistics of the institutes concerned any mention of these unfortunate cases."

So long back as January 1, 1920, Pasteurian statistics were criticised in the *Times*, by no less an authority than the eminent statistician, Professor Karl Pearson, well known as the Galton Professor of Eugenics and Director of the Laboratory for National Eugenics at the London University. Questioning the boast of Pasteur's "conquest of hydrophobia," he wrote:—

"Full statistical data for the Pasteur treatment both in Europe and Asia are not available. What data are published

[1] Publications of the League of Nations. III. Health. 1927. III. 14.

permit of no prudent statistical judgment. If the Indian Government is in possession of information on this point, why is it withheld? If it does not possess it, why does it not obtain it and issue it? Is there any cause for dissatisfaction with the results obtained, and have any changes been made in the treatment on the basis of such dissatisfaction or for any other reason? These are questions for which answers should be demanded in the House of Commons. No Government is to be blamed for adopting a course recommended by its scientific advisers. But it sins not only against science and humanity, but against the brute world as well, if it does not provide the material it must possess for a judgment of the success or failure of its efforts. In our present state of knowledge I venture to assert that it is not wise to speak of the 'conquest of rabies.'

I am, Sir,
Yours,
KARL PEARSON.

University College, W.C.1."

Such is the expert statistical commentary that after all these long years replies to Pasteur's request to await the verdict of time and of experience.

Even the information obtainable from the Pasteur Institutes can hardly be encouraging to believers in Pasteur's treatment. For instance, if we turn to the Reports of the Pasteur Institute at Kasauli in India, we find the big increase from 10 deaths from hydrophobia in 1900, to 72 deaths in 1915. Against this we can scarcely set the corresponding increase in cases, because so many of the latter cannot be described as genuine; it is frankly acknowledged in the Sixteenth Annual Report[1] that many of the Europeans have undergone no risk whatever. We can well believe this when we recall the example of Lord and Lady Minto, who went through the course of inoculations merely because their pet dog had been bitten by another dog, supposed to have been mad! A large proportion of the Indians can run no risk either, except from the treatment, seeing that the patients, according to the Report's own showing, have not all been bitten, but many merely "scratched," or "licked,"

[1] p. 21.

and not all by rabid, but many by merely "suspected" animals. Moreover, these animals include human beings, cows, calves, pigs, deer, donkeys, elephants and almost every known species! Between the years 1912 and 1916 there were 114 patients who had been bitten by horses, and 80 who had been the victims of human bites! Thus we see that in a considerable number of so-called "cures" there is no pretension to the patients ever having run any risk from actual mad dog bites.

In an interesting Note, this Sixteenth Annual Report[1] recommends "the use of atropine[2] in cases which have developed symptoms of rabies." It goes on to say:—"The use of this drug was suggested to us by Major F. Norman White, I.M.S., to whom we acknowledged our thanks. Its effect is to relieve throat spasm, and if it be given at suitable intervals, this distressing symptom can be entirely obliterated, with the result that the patient is able to eat and drink. Apart from this beneficent effect, there is always in the background the hope that in certain cases, throat spasm (which is the proximate cause of death) might be held in check until the phase of recovery had set in. . . . *Clearly the most hopeful cases would be those of the untreated*, in which the incubation period was naturally a long one. . . ."

So here we find Pasteurian workers themselves acknowledging a possible *cure*, which has no connection with Pasteur, and, on their own admission, it is as likely as not to be more profitable without the addition of his treatment.

For the matter of that, hydrophobia has never been a complaint without a remedy, even after the paroxysms have set in. Pilocarpine, a drug which induces profuse sweating, has been known to cure cases; while, on a similar principle, Dr. Buisson of Paris, author of a treatise, *Hydrophobia, Preventive and Curative Measures*, cured himself of an attack by the use of a vapour bath and inaugurated a

[1] p. 35.

[2] "We have found the 1/100th grain of the sulphate, injected subcutaneously every four hours, is usually sufficient to obliterate spasm." *Kasauli 16th Annual Report*, p. 36.

remedial system, named after himself, which has been most successful,[1] and which, in case of need, can be put into operation at the Battersea General Hospital (Anti-Vivisection), London, S.W.11.

It is, to say the least of it, remarkable that definite curative measures should be overlooked and set aside for a mere preventive which cannot set forward a single tangible proof of ever having saved anyone, while, on the other hand, as we have seen, there is undeniable evidence that it has occasioned a new complaint, paralytic hydrophobia. For such procedure there must be some explanation and perhaps the Indian paper, *The Pioneer*, for March 12, 1919, unconsciously provides it:—

"The Central Research Institute[2] at Kasauli has developed its vaccine production to an almost incredible extent. The yearly average before the war was 18,500 cubic centimeters; during the war it rose to over 2½ million cubic centimeters, and included anti-typhoid, cholera, pneumonia and influenza vaccines. From a monetary point of view alone the value of the Kasauli vaccines for the period of the war was about half a million sterling."

Pasteur's inoculations for hydrophobia form part of a vast money-making system, in which the beneficiaries have no wish that any item should be discredited. The Kasauli returns are only a fraction of the monetary gains accruing in Europe, Asia and America. A few years back, we were told by Professor Ray Lankester that the Lister Institute in London made £15,800 a year by the sale of vaccines and sera—a sum that seems likely to have increased largely. Thus we find science dominated by commercialism. Were it not for pecuniary advantages, there seems little doubt that the broth emulsions of spinal cords would have gone the same way as an older less nauseous panacea—"the hair of the dog that bit you"! From the earliest records of history, the prevalent mania

[1] For cases of cures, see *On Rabies and Hydrophobia*, by Surgeon-General Thornton, C.B., M.B., B.A.

[2] A separate institution from the Pasteur Institute.

seems to have been for "frightfulness" in medicinal remedies; but the witches' cauldron itself never surpassed the noxious nostrums inaugurated by Pasteur in what has proved indeed "a new era in medicine." It is the era for the injection into the blood of matter of varying degrees of offensiveness, the era in which animal experimentation, vastly increased, has found its sequence in experiments on human beings, and the credulous and the ignorant are everywhere at the mercy of the subcutaneous syringe and thereby swell the monetary returns of the manufacturers of vaccines and sera!

CHAPTER XVIII

A Few Examples of the Cult in Theory and in Practice

What a striking contrast between Louis Pasteur, the worn, paralysed man aged before his time, and the magnificence of the Institute erected in his honour and called after him, which was opened on the 14th November, 1888, at Paris! For the ambitious chemist had achieved his goal—fame and fortune. He now found himself installed as the idol of medical orthodoxy, and through succeeding years his worshipful followers were to waft his doctrines abroad like incense to his memory.

The reason for the general public's acclamation of his views has been succinctly explained to us by Béchamp in the preface to his work *La Théorie du Microzyma*. Here he writes:—"The general public, however intelligent, are struck only by that which it takes little trouble to understand. They have been told that the interior of the body is something more or less like the contents of a vessel filled with wine, that this interior is not injured—that we do not become ill except when germs, originally created morbid, penetrate into it from without, and then become microbes. The public do not know whether this is true; they do not even know what a microbe is, but they take it on the word of the master, they believe it because it is simple and easy to understand, they believe and they repeat that the microbe makes us ill without inquiring further, because they have not the leisure, nor, perhaps, often the capacity, to probe to the depths that which they are asked to believe."

On the other hand, experts have been educated from the start to consider micro-organic life from the Pasteurian standpoint and to accept these theories as though they

were axioms. Thus it is, perhaps, understandable why it is only from an unbiased vantage-ground that the contradictions of the germ-theory of disease are seen to make it ridiculous. Its rules, the postulates of Dr. Robert Koch, state, *inter alia*, that a causative disease-germ should be present in every case of a disease and never found apart from it. What are the facts? One of the original props of Pasteurian orthodoxy, the Klebs Loeffler bacillus, arraigned as the fell agent of diphtheria, was, by Loeffler himself, found wanting in 25 per cent of the cases; while, on the other hand, it is constantly revealed in the throats of healthy subjects, since, as Béchamp explained long ago, a bacterial evolution of microzymas is not necessarily noxious.

The followers of Pasteur, however, have their method of overcoming the theoretic difficulty, namely, the carrier-theory, by which healthy people are accused of propagating certain "germs," which they are supposed to disseminate. This accusation has been brought against those who have never in the whole course of their lives suffered from the complaints that they are accused of distributing; while, in one noted case, that of a certain cook, Mrs. Roberts, of Wrexham, whose microscopic inhabitants were said to have dealt out intestinal trouble, it was found that she had never seen, much less touched, the pork pies described as the delivery medium of her murderous microbes.[1]

In their *Manual of Infectious Diseases*, Goodall and Washbourn[2] state:—"Enteric fever differs from other infectious diseases in not spreading directly from individual to individual. There is thus but little danger in visiting patients suffering from the disease."

Yet, while actual victims of the fever are pronounced innocuous, no hesitation is shown in accusing healthy persons, some of whom have never undergone the complaint, of being promoters and disseminators of it.

[1] Some twenty cases of an illness, called *para-enteritis*, with four deaths, were ascribed to the consumption of these pork pies, which Mrs. Roberts was accused of having infected.

[2] 1st ed., p. 293.

The carrier-theory is also constantly invoked in connection with diphtheria. Years ago we read[1] of the throats of 700 schoolchildren at Alperton in Middlesex being examined, with the result that 200 were accused of being diphtheria-carriers and were isolated in consequence. One outstanding weakness of the theory is that we never seem to hear of the isolation of prominent bacteriologists, who obviously should set the example in undergoing microscopic and chemical tests and the subsequent quarantine, so far, apparently, only advocated for other people! But, as the Editor of the *Lancet*[2] has confessed, without the carrier-theory Koch's postulates could not even pretend to be fulfilled.

Take, for instance, the fourth postulate, which describes the causative germ as provocative in an animal of the same disease as that with which it was originally associated. We are told in the same article in the *Lancet*[3] how the pneumococcus of pneumonia introduced even into the lung of a rabbit brings about not pneumonia, but general septicæmia. According to Béchamp's theory of the differences between the microzymas of varying species, this result is understandable and presents no mystery; but it means the undoing of the truth of Koch's fourth postulate.

In Sternberg's *Text-Book of Bacteriology*,[4] we find:—"The demonstration made by Ogston, Rosenbach, Passet and others, that micrococci are constantly present in the pus of acute diseases, led to the inference that there can be no pus formation in the absence of micro-organisms of this class. But it is now well established by the experiments of Crawitz, de Bary, Steinhaus, Scheurlen, Kaufmann and others that the inference was a mistaken one and that certain chemical substances introduced beneath the skin give rise to pus formation quite independently of bacteria."

On the other hand, Dr. Robb[5] has shown that under the

[1] See the *Evening News* of the 4th June, 1920.
[2] March 29, 1909. [3] *ibid.*
[4] p. 371 (1901). [5] *Aseptic Surgical Technique*, by Hunter Robb, M.D.

most rigid antiseptic treatment, micro-organisms are constantly found attached to sutures when removed from wounds made by the surgeon, and that a skin abscess is frequently associated with the presence of the most common of these micro-organisms, e.g., *staphylococcus albus*.

Thus, on the one hand, we are given evidence that pus formation may be independent of bacteria, while on the other, the utmost precautions against micro-organisms may not prevent their presence. From the viewpoint of Pasteur this is a contradiction not easily accounted for by his theory of invasion. We are told by his son-in-law[1] that it was his habit to speak of an *invaded* patient. Yet, we have just been informed, on the one hand, of pus without any so-called microbes, and, on the other, of microbes when every precaution has been taken against them. This is very confusing according to Pasteur's teaching. On the contrary, we find explanation directly we turn to Béchamp. According to his doctrine, which, with the cautiousness of a true man of science, he put forward as a probable hypothesis, instead of asserting it to be a proved fact, "incapable of question," after the example of Pasteur, it seems possible to understand the malignant influence of certain chemical substances upon the normal microzymas of the body and the pus formation that might be the consequence. In the other example, where micro-organisms are seen, in spite of antiseptic precautions against external invasion, we are shown the apparent accuracy of Béchamp's view that the medium having become unsuited to normal microzymas, they themselves develop into bacteria, thus proving the latter to be the consequence, instead of the origin, of the disease-condition.

Another remarkable theory that has had to be invoked in support of the general germ-theory is that of Metchnikoff's phagocytosis, or the assumption that the leucocytes, otherwise the white corpuscles of the blood, are in effect its scavengers which make an end of undesirable intruders. A favourite term for them has been that of the police of

[1] *The Life of Pasteur*, p. 291.

the body, notwithstanding the salient fact that the more of them the less the body seems safeguarded, while it gains in security with the diminution of this hypothetical police force. Béchamp taught that the leucocytes are living, but he treated Metchnikoff's theory with ridicule. "The leucocytes," he wrote, in *Les Grands Problèmes Médicaux*, "are even held to be so much alive that they are represented as pursuing the microbe to swallow and devour him. The droll thing is that they believe it!" But without phagocytosis what would become of the whole doctrine of invasion and resistance and all the other popular theories?

One probable factor of satisfaction in the disease germ-theory has been the explanation that it has been supposed to provide for the problem of infection. It is so easy to conjure up arrays of malignant microbes passing from one diseased subject to another. Such an idea seems to be prevalent even with men of science. For instance, we find that before the Royal Commission on Vivisection, Dr. C. J. Martin, of the Lister Institute, is reported to have stated[1]:—"His (Pasteur's) experience on this subject (fermentation) led him to the great generalisation that infectious diseases might themselves be interpreted as particular fermentations, and as due to specific micro-organisms. By a series of masterly experiments on animals, he established the truth of his hypothesis in the case of anthrax and chicken cholera and swine erysipelas. These results of Pasteur's may be regarded as the foundation of the whole modern study of contagious diseases both in man and in animals; and their extension by Pasteur and his pupils, and by bacteriologists and pathologists all over the civilized world, has led to the discovery of the causation of most of the infectious diseases to which man is liable."

We have already compared Béchamp's and Pasteur's work on fermentation, and in regard to Pasteur's "masterly experiments on animals," we have seen something of "the truth of his hypothesis" in the case, for instance, of

[1] See *Final Report of the Royal Commission on Vivisection*, p. 29.

anthrax. Finally, in respect of the most infectious diseases, such as scarlet fever, measles and smallpox, no specific micro-organisms are found in association, though that is no hindrance to the Pasteurian in claiming that they are there all the same, but are ultra-microscopic, even if this be hardly in accord with the "cautiousness" advocated by Pasteur. As Professor Béchamp once said:—"If virulent germs were normal to the atmosphere, how numerous would be the occasions for their penetration independently of those by way of the lungs and intestinal mucus! There would not be a wound, however slight, the prick even of a pin, that would not be the occasion for inoculating us with smallpox, typhus, syphilis, gonorrhœa."

In regard to this, we will quote a passage from Mr. Alexander Paul's summary of the preface to *La Théorie du Microzyma*.[1] Mr. Paul writes as follows:—"M. Béchamp argues that if the simple or evolved microzymas, which may be found in certain humours of the body, came from the air and penetrated so easily the cells of the human body, there is one humour, in ceaseless contact with the air we breathe, in which we should find them always the same in all animals. This is the saliva of the mouth. It is found, however, that the properties of human saliva and that of other animals are different. M. Béchamp says that the epithelial cells, the microzymas, and the bacteria of the tongue of man have a special chemical action personal to themselves, and altogether different from those of the tongue of the cow or the pig, the horse or the dog. Now, if the germs of the air do not operate to modify the function of a humour which is so unceasingly, so largely, and so directly in contact with the common air, it is difficult to understand how they operate to modify the functions of the inner tissues and humours protected by insurmountable barriers."

Were it not that the art of thinking is so rarely practised, reflections such as these might, surely, have demonstrated

[1] See *The Vaccination Inquirer* for February, 1909, p. 178.

long ago something to be at fault with the Pasteurian view of the germ-theory. And, even in cases where the germ hunter seems most sure of his microbe, in a little while what dire confusion is apt to overtake his certainty. Never, for instance, did there seem to be a better bolstered case than Sir David Bruce's arraignment of the *micrococcus melitensis* in goat's milk as the cause of Malta fever. Yet when Dr. Walter R. Hadwen, of Gloucester, took up the defence,[1] how innocent after all, he proved the supposed offender. The decline of the fever in the Navy was found to have had nothing to do with abstinence from goat's milk, but to have been gradual and to have coincided with the dredging of the harbour at Malta. Neither was the sudden drop in the Army disease-rate to be accounted for by avoidance of the milk, for it had already taken place, before that beverage was banned, when the troops were mostly removed from the insanitary St. Elmo barracks to new quarters at a higher altitude. To these measures the improvement in our sailors' and soldiers' health-rate was clearly traced by Dr. Hadwen's investigations, and the main effect of the *micrococcus melitensis* was to gain a knighthood for its false accuser, while, incidentally, it occasioned a great deal of discontent among Maltese connected with the milk industry. Dr. Agius of Malta, who, at the time, went into the matter very thoroughly, found that bad sanitation was invariably the cause of outbreaks of fever in private houses, which were, sometimes, the quarters of British officers. On one occasion, it was only after a floor had been taken up that the real seat of the trouble was discovered.

Yet, upon a theory so constantly found at fault when thoroughly sifted, there has been erected a whole system of inoculation. Or, perhaps, the facts may be stated conversely. Had it not been for the sale of sera and vaccines, nowadays grown to such vast proportions, Pasteur's germ-theory of disease might before this have collapsed into obscurity. Thus it can hardly be denied that he com-

[1] See *The Contemporary Review* for August and November, 1909.

mitted an offence in dragging medical science down to a commercial level. Moreover, he has besmirched its fair name by allying it with cruelty. It is true that also in this he was an imitator. He was the friend of men like Claude Bernard, who, in the words of Professor Metchnikoff,[1] "feel no scruples in opening the bodies and submitting the animals to the most cruel sufferings." But, atrocious as is often their torment, victims of the knife were and are few in number as compared with the millions of victims in pathological laboratories, sometimes undergoing tests as fantastic and misleading as they are cruel, since they could never furnish real evidence of disease under natural conditions.

As examples might be instanced birds and rats in minute cages, slowly devoured by fleas, by way of proving whether the latter can convey sleeping sickness, without regard to the fact that the inevitable bad health of creatures thus tormented cannot with certainty guarantee anything except the callousness of their tormentors. Or again, the test of milk by its inoculation into guinea-pigs, which, kept in covered tins, would by the mere fact of such unhealthy captivity, be made liable to tuberculosis. Yet for this, the ratepayer dips into his pocket, while, for all he can tell, the milk he consumes may have come from a consumptive cow wading through a filthy farmyard and milked by a diseased individual into a dirty utensil. Hygienists in some parts avert such conditions, leaving Pasteurians to worry the guinea-pigs. The amount of harm that has ensued from the diversion of attention from real to false factors in the causation of ill health is probably incalculable. An example, in this connection, in regard to plague in India, is the amount of time and money wasted over fleas and rats that might be expended upon the insanitary huts standing on filth-sodden soil, which Dr. Charles Creighton, in a treatise on the subject,[2] has clearly shown to be breeding grounds of the pestilence,

[1] *Les Annales*, Paris, April, 1908.
[2] *Plague in India*, by Charles Creighton, M.D.

To return to the subject of milk, admirers of Pasteur may point in pride to the preservative methods called by his name, which immortalise his memory; but even here the praise is so faint as to be damning. If we turn to the *Journal of the Royal Society of Arts* for September 19, 1919, we find an article on "Problems of Food and our Economic Policy," by Professor Henry E. Armstrong, Ph.D., LL.D., D.Sc., F.R.S. Here we are told that "the great reformer of recent times has been the chemist Pasteur—the extent to which he has influenced our doings is astounding." Professor Armstrong then shows how, owing to him, "wines were sterilised and the *Grand Vin*, the result of some fortuitous concourse of organisms, became a great rarity; the quality of wines was thereby reduced to a low general average, though of course much was saved from the sewer. Beer suffered a like fate, though on the whole the changes were much to the public advantage. But *the real harm was done*[1] when milk was tampered with. . . . Dilution became a general practice; the public suffered less from occasional dishonest tradesmen, but it was deprived of the advantages up till then derived from dealing with the large body who were honest purveyors of the natural article. The blow was made all the heavier by the introduction of clever engineering appliances for the separation of the cream. Then Pasteur's teaching became operative once more, aided this time by Koch; milk was not only diluted but also sterilized. Some lives may have been saved but *the step has undoubtedly been productive of untold misery.*[2] Not a few of us have long held, on general grounds, that a material produced as milk is cannot be heated above blood-heat without diminishing its dietetic value. Recent observations show indeed that the anti-scorbutic advitant, which is none too abundant a constituent, is affected, although apparently the fat-soluble anti-rachitic and water-soluble anti-neuritic factors are not destroyed; but difficulties have been encountered in localities where the milk supply has been systematically sterilised and it may

[1] Italics ours. [2] Italics ours.

well be that it suffers in quality in ways not yet elucidated. The inquiries thus far held into the effect of sterilising are in no way satisfactory and are open to criticism on account of their incompleteness and unscientific character. The risks from typhoid and other similar infections are now slight and the main object of sterilising milk is to secure the destruction of the organism which conditions tubercular disease. But it may well be that in destroying some one or other mysterious constituent of the advitant class, the food value is so lowered that effects are produced which render the system specially sensitive to tubercular infection; such infection seems always to be with us apart from milk. Moreover, when milk is sterilised, the lactic organism is destroyed and it becomes a particularly favourable nidus for the growth of putrefactive organisms: it is therefore a potent cause of infantile diarrhœa."

Thus, the verdict of time and unbiased criticism continue to pronounce judgment upon the works of Pasteur. But if the mere consumption of impoverished food can be believed to be so injurious to the consumer, what must be the effect of the deluge of sera and vaccines introduced directly into the blood-stream?

In spite of the modern medical mania for inoculations, a remarkable ignorance on the subject prevails among those most ready to submit to this fashionable mode of experiments on human beings. Many can scarcely distinguish between a serum and a vaccine.

Serum, the colourless part of the blood, is usually, for inoculatory purposes, taken from the blood of a horse into which disease matters have previously been injected. The strength of this serum is generally tested upon guinea-pigs, that is to say, by their recovery or death from the sickness involved by its inoculation under standard conditions into their bodies. Animal suffering comes into play in this connection from start to finish, while, as regards the human race, considering the danger of the introduction of the serum of one species into that of another, it is, perhaps, fortunate that serum-therapy, although originally

acclaimed as the panacea for all ills, has yielded in popularity to vaccine-therapy.

The latter, needless to say, has no connection with cows. Under Pasteur's tutelage, precision in nomenclature was lost as much as precision in theories. The name "vaccine" is now applied to micro-organisms and their surrounding medium abstracted from a sick body, the organisms being left to multiply in a suitable nutritive substance, known as a "culture," afterwards being usually killed by heat and prepared in various ways, according to the prevalent fashion. The nostrum is finally sold as a cure, or, more often, a preventive against the disease with which the micro-organisms were originally associated. In this case, animals are spared a part in the preparation, though, owing to their use as tests, suffering is for them by no means necessarily eliminated.

We are here reminded of the homeopathic law of cure, that of "Like cures Like," though what a contrast is presented to Hahnemann's scientific precision in allowing for individual idiosyncrasies. Whereas he submitted his drugs to Nature's laboratory, the stomach, according to the Pasteurian system, on the contrary, an introduction is made directly into the blood, regardless of Nature's precaution—the efficient coverings wherewith she has protected this life-stream against external intrusions. It has indeed became the fashion for puny humanity to consider itself wiser than—choose which name you will—Nature or Providence.

We are well aware of the array of statistics with which Pasteurians confront the critics of the system of inoculation, and in reply we would say that statistics are worthless to prove results without full investigation and thorough allowance for the conditions of their presentment. For instance, it is easy to parade a fall in the diphtheritic fatality-rate since the introduction of antitoxin. Yet that fall does not conduce to the merits of the serum, if seen merely to be the result of a case-rate inflated by a bacteriological as opposed to a clinical diagnosis and

the inclusion as diphtheria of what in the past would have been considered to be mere sore throat, tonsilitis, laryngitis, etc. The altered diagnosis, in itself, prevents proper comparison between past and present case-rates. But if, with an inflated case-rate, there is an increase, instead of decrease, in the *death-rate*, such an increase in surely highly significant. For instance, we find that, for the 15 years subsequent to the introduction of anti-toxin, the number of deaths in England and Wales from diphtheria became 20 per cent greater than they had been for the 15 years prior to the serum-treatment.[1] Though the Metropolitan Asylums Board's reports of cases may seem to show at first sight by a decreased death-rate that advantage accrues from the use of the anti-toxin, their detailed particulars confirm an opposite opinion. Whereas for the years 1895 to 1907 there were 63,249 cases of diphtheria treated with anti-toxin, of which 8,917 died, giving a fatality-rate of 14.09 per cent, there were for the same years 11,716 cases *not* treated with anti-toxin, of which only 703 died, giving a fatality-rate of 6 per cent. Foot-notes to the tables show that of the latter cases 55 were moribund when admitted and 12 died of diseases other than diphtheria, so that the exact fatality-rate should in reality be under 6 per cent. It is to be regretted that the cases treated with and without anti-toxin are no longer differentiated in the Metropolitan Asylums Board's Reports and since 1930 the Board itself has ceased to exist. From those cases that have been particularised there seems to be no gainsaying the belief that the improved methods of nursing and medical treatment, which should reduce deaths, accomplish this only in a lessened degree when anti-toxin is administered. The following table supplies a proof of this view in regard to infantile diseases. We see here the remarkable decrease in measles, scarlet fever, and whooping cough, complaints

[1] This calculation is based on the years 1880-94 as the pre-anti-toxin period. Were the comparison made from 1879-93, the increase would amount to 33.88%. The Registrar-General gives small support to the allegation that many of the earlier croup deaths should have been classified as diphtheria.

not subject to treatment by inoculation; while diphtheria, with its specific anti-toxin, shows an increase of 102 per million. The contrast is surely striking.

[1]ANNUAL MORTALITY PER MILLION LIVING AT AGES 1-5 YRS. IN 1911-14 AND 1916—BOTH SEXES

	Death-rate		Increase (+) or Decrease (—) between 1911-14 and 1916	
	1911-14	1916	+	—
6 Measles	2,643	1,225	—	1,388
7 Scarlet Fever	369	227	—	142
8 Whooping Cough ..	1,202	1,050	—	152
9 Diphtheria	769	871	102	—

The claim for immediate injection and the advantage of a first day inoculation as compared with a second day, and so forth, may surely be dismissed for the following reasons. Before clinical symptoms are manifest it is impossible to tell whether the trouble would ever be serious, if indeed the diphtheria be genuine; and if, on the other hand, it be asserted that the prompt administration of anti-toxin has prevented dangerous illness, it is as easy to assert, on the other, that through anti-toxin a mere mild sore throat has been aggravated into severe sickness, sometimes complicated by heart trouble and paralysis. The one method of argument is no more inexact and unscientific than the other.

Also, one may ask why, if diphtheria anti-toxin be such an unfailing remedy, it should have been found necessary to introduce the Schick system of preliminary test and subsequent immunisation. The supposedly susceptible children should run small risk if provided with an infallible cure. If, in answer to this, it be argued that the immunisation is for the prevention of diphtheria for all time, it may be retorted that statistics show no im-

[1] Part of Table XXXIV on page xiv of Registrar General's Report for 1916 (England and Wales).

provement upon natural immunity; while, moreover, in many cases the preventive has proved far more dangerous than the disease.

		Injured by Diphtheria Immunisation	Killed by Diphtheria Immunisation
Dallas, Texas	1919	60	10
Connecticut	1924	45	—
Baden, Austria	1924[1]	many	6
Russia	1927	—	12
China	1927	37	5
Bundaberg, Queensland	1928	5	12
Medellin, Columbia	1930	many	19

An even greater tragedy of Pasteurian preventive methods is the murder of innocents at Lübeck, during the early summer of 1930, from B.C.G., or the Calmette Tuberculosis Vaccine, a culture administered by the mouth to newly born infants. The Health Department of the City made an emotional appeal to parents to allow the immunisation of their children whether likely to grow up in a tubercular environment or otherwise. Of the 253 babies subjected to the Calmette treatment 69 died from it and 130 were made seriously ill. In view of such a calamity, it is not surprising that the *Reich* Health Office decided that such prophylactics were not to be recommended, and the *Reich* Health Council "considers an extension and tightening up of the existing regulations for the production, issue, and employment of vaccines of all kinds to be desirable."[2]

Finally, we have to remember what wonderful statistical boasts have been demolished when genuine epidemics have made their appearance. For some considerable time, one of the trump cards, so to speak, of the Research (Vivisection) Defence Society was the anti-meningitis serum of Dr. Flexner and Dr. Jobling of the Rockfeller Institute, New York. Remarkable statistics were produced

[1] Now forbidden.
[2] *The Times*, 15th December, 1930.

without explanation that convenient omissions had brought about these seemingly magical returns. The serum, first tried in the Spring of 1907, was acclaimed as bringing about a "complete revolution." Yet what about this wonderful cure when a terrible outbreak of meningitis in New York, with a death roll of 745 for the single month of July, 1916, transformed the American capital into a city of mourning? Flexner's marvellous serum was so inefficacious that we find it barely gained a mention, and its discoverer confessed that "there exists at present no specific or curative treatment."

It transpired, further, that this complaint, known also as spotted-fever, is, at any rate according to bacteriological diagnosis, fast losing its limitation to childhood. Outbreaks of it are said to have been frequent among young men in military training camps. It has followed so suspiciously in the wake of anti-typhoid and other inoculations that, instead of such measures having provided safeguards for health, it would seem far more probable that they have sometimes been directly provocative of sickness. And now this brings us to a few lessons that we may be able to derive from the inoculatory experiments that were practised upon our fighting men during the course of the Great War.

CHAPTER XIX

Some Lessons of the Great War

It is constantly asserted that the comparative freedom from epidemic disease among the armies fighting on the western front, during the war, is a sufficient demonstration of the value of "preventive" inoculations. We, on the contrary, believe that a study of the subject proves such an opinion to be based upon superficial observation. It has to be remembered that every sanitary and hygienic precaution possible to call into being was attended to on the western front.

And here we may pause to notice that the Great War was not without accompanying epidemics, affording an interesting illustration of the substitution theory of disease-conditions, to which we have already alluded.[1] Throughout history we find that plagues have followed in the wake of war, with a systematic diminution in intensity according to the sanitary and hygienic conditions of the population. Thus the black death of the Middle Ages was, in later times, replaced by smallpox which, in our own day, has found its substitute in mysterious outbreaks of influenza. In reference to the Great War, we read as follows[2]:—"The war ended with the accompaniment of the influenza epidemic of 1918-19 (as that of 1870-71 ended to the accompaniment of pandemic-smallpox)—an epidemic which, without reckoning South America, China, Japan and great tracts of Asia and Africa, is computed to have claimed eight million lives." Thus no one can deny that the war involved the inevitable aftermath of disease, whose far-reaching ravages may perhaps be explained by

[1] See Chap. XIII of present work.

[2] See *Report on Influenza of Chief Medical Officer to the Ministry of Health*, p. 46.

the distribution of campaigns in widely diversified areas.

To return to the subject of inoculation, its success as a preventive of disease can only be tested under conditions where sanitary and hygienic measures fail, and as, whereever these were wanting, whether in East Africa, Gallipoli, Palestine or Mesopotamia, disease-conditions ran riot, we confess that we entirely fail to see where the success of inoculation came in.

Nevertheless, the Press is inundated with medical arguments such as the following statement by Lieutenant-Colonel S. A. M. Copeman, Officer-in-Charge of the R.A.M. College, which appeared in *The Times* for February 15th, 1917. "As to typhoid fever, contrasting admissions to hospital and deaths in the South African campaign and in France for the first two years of the war, there had been a marvellous effect of prophylactic inoculation in the prevention of attack, and to an even greater extent in the saving of life. A similar result had followed the later introduction of inoculation in the French Army, which suffered heavily from typhoid fever in the early months of the war."

No better criticism of the above can be found than that of Mr. E. B. McCormick in the *Vaccination Inquirer* for March, 1917. He writes as follows:—"The implication here is that as between the South African war and the European war essential conditions were similar apart from inoculation in the present campaign. Now nobody denies that sanitary conditions are a governing factor, or at least an important one, in the prevalence of typhoid. It is notorious that sanitary conditions were deplorable in South Africa, whereas in France they have been, in Sir Frederick Treves' words, without a parallel in the history of war. What are we to think of medical logic which (in its special pleading for inoculation) continues to ignore this vital factor? When we remember further that the two campaigns are not even fully differentiated in respect to inoculation, but that 400,000 doses of Sir Almroth Wright's poison were sent to South Africa for the Army, and that

in the first part of the campaign in France inoculation was hardly practised at all amongst British troops, the grotesque inadequacy of Lieutenant-Colonel Copeman's line of argument is apparent. That his accuracy on points of fact is on a par with his logic appears from his suggestion that the introduction of inoculation was later in the French Army than in ours, whereas the fact is that it was not only earlier but was made compulsory by law in 1913, whereas ours is still nominally optional. The admission that the French Army suffered heavily from typhoid in the early months of the war is therefore worth noting."

Where we can make something of a comparison is in respect to the Japanese troops, who, in the Russo-Japanese war, inaugurated the sanitary and hygienic measures that have since been followed in the European war and were rigorously carried out on the western front. As regards inoculation the conditions are diametrically opposite. At the time of the Russo-Japanese war, it was definitely stated that:—"No prophylactic inoculations are being practised in the Army with regard to enteric fever. Professor Kitasato has advised them, but the Army Medical Authorities refuse to allow them until they are better satisfied as to the results of Wright's prophylactic treatment."[1] Yet among those uninoculated troops the cases of enteric numbered only one-sixth of those that occurred among the partly inoculated British troops in the Boer war. The Japanese cases were almost entirely in the First Army, in which sanitary and hygienic regulations were less attended to; whereas in the Second and Third Armies enteric was almost eliminated, although these Armies were uninoculated. This Japanese experience surely upholds the argument that sanitary and hygienic precautions, not inoculation, deserve credit for the good health-rate on the western front.

Foremost among safeguards for the health of the troops

[1] *The Russo-Japanese War Medical and Sanitary Reports*, p. 360. See also *Anti-typhoid Vaccines*, by L. Loat, published by The National Anti-Vaccination League, 25, Denison House, 296, Vauxhall Bridge Road, Westminster, London, S.W.1.

was, undoubtedly, the care exercised in regard to the water-supply. On occasional houses in the outskirts of Lille and along the Menin road, German notices still remain[1] to indicate where good drinking water may be obtained and to illustrate Teutonic attention to details. The history of water-purification for our own troops has been described by Captain J. Stanley Arthur, R.A.M.C. (T.F.) in a paper read before the Institution of Mechanical Engineers on November 19th, 1920, and published in *The Engineer* for November 26th and December 3rd, 1920. Here we are told how "bleaching powder, or chloride of lime, was first used to sterilize a supply of drinking water in 1897 at Maidstone, where an epidemic of typhoid was raging. Its use was attended with very successful results, typhoid being rapidly stamped out." Further, we read that "Chlorine in the gaseous condition, although used in America to a small extent for some time, has only come into general use during the last few years. The amount of chlorine, either as a gas or from bleaching powder, required to sterilise water is quite small. . . . At the outbreak of the war the only method of water purification, other than that involving the use of tablets of acid sodium sulphite, that could be carried out in the field was embodied in the water cart. . . . Attempts were made to devise a simple method by which the amount of bleaching powder required to sterilise any water could be determined in the field. The first suggestion was made by Professor Sims Woodhead and the actual details, resulting in the fitting up of a case containing the necessary apparatus and chemicals with instructions for carrying out the test, were worked out at the Royal Army Medical College under the direction of Sir William Horrocks. With this test case, known in the Army as 'the Case Water Testing Sterilisation,' and the water cart as the starting point, the whole of the great water purification scheme of the Army has been built up. That the methods adopted have been successful is seen from the fact that throughout the war there has

[1] August, 1922.

been no epidemic of any water-borne disease." Captain Arthur goes on to speak of advances upon the water cart and also of work done in America for the administration of chlorine gas to water for sterilisation purposes. The two types of chlorinators constructed by Messrs. Wallace and Tiernan of New York have proved most satisfactory and their direct feed type was "adopted throughout the water purification plants in use in the British Army." The article treats further of stationary and portable plants and the whole process of purification. Captain Arthur also mentions the difficulty of supplying sterilised water to the troops in the East in the early days of the war; but shows that now "a supply of sterilised water can be maintained under almost any possible conditions by use of one or the other of the various types of water purification plants mentioned," and he tells of the new plants ordered for use in the East. To this system of water purification he ascribes all the praise for the Army's good health-rate. That this is the case is evident from the contrasting sick-rates on all those fronts deprived of similar advantages. With a contaminated water supply, inoculation proved no preventive of disease. And if inoculation, unnecessary under safeguarded conditions, is useless when such conditions fail, of what use is it at all?

Inutility, however, is not the only, or the most serious, criticism to be levelled against the practice; the teachings of the Great War point to it as directly deleterious.

In a pamphlet, *Microbes and the War*,[1] by Dr. Walter R. Hadwen, we find a quotation from Professor Ernest Glynn as follows:—"Sickness (in the South African campaign) was responsible for the loss of 86,000 men by death and invaliding (in nearly three years); yet the total number of officers and men, including native Indian troops, leaving the Gallipoli Peninsula on account of sickness from April 25 to October 20 may be stated as 3,200 officers and 75,000 other ranks! The total has since been increased to 96,000."

[1] Published by the British Union for the Abolition of Vivisection, 47, Whitehall, London, S.W.1.

"In short," comments Dr. Hadwen, "the toll of disease and death in these modern days of serums and vaccines with all their 'protecting' influences against microbes, was, in proportion to the period and the respective number of troops employed, nearly six times greater in the last six months of the Gallipoli disaster than in the whole three years of the Boer War."

The following official figures for the losses in the Gallipoli Expedition speak for themselves.

Killed	25,270
Wounded	75,191
Missing	12,451
Sick	96,684

Taking into consideration the shot and shell from which there was no escape in that inferno of fighting, this enormous number, 96,684 victims of disease, is nothing short of amazing; especially, too, in view of the fact that so many Australasian troops were included, representing the pick of robust manhood. The sick far outclass the number killed and even the number wounded; and we have to remember that of this great host of invalids almost every man had been rigorously inoculated. The nomenclature applied to their complaints is a mere minor matter in face of the sweeping generalisation that the application of Pasteurian methods on a vast scale met with an overwhelming return in the shape of illness. Indeed, so high was the sick-rate among the stalwarts of Gallipoli that the inference is permissible that inoculation conduced towards it by poisoning the systems and lowering the vitality of the fighting men.

In spite of this general damnatory evidence, bacteriological diagnosis has done its utmost for statistical inoculatory success by giving every name except typhoid to intestinal troubles, which, by clinical diagnosis, in previous wars, would have been thus classified. The process of bacteriological diagnosis has been illuminatingly divulged by Lieutenant-Colonel C. J. Martin and Major W. G. D. Upjohn, Pathologists of No. 3 General Hospital,

A.I.F.[1] The exceedingly doubtful agglutinin reaction was the method adopted, and, with a candour as delightful as it was unconscious, these gentlemen confessed that in patients "previously inoculated" the development of typhoid agglutinins was regarded "with suspicion." They went on to say that they "only diagnosed typhoid when the typhoid bacillus was isolated or when, the case being clinically typhoid, no paratyphoids could be detected."

The Vaccination Inquirer[2] (Mr. E. B. McCormick), in criticising the Report, remarks:—"Thus the mere presence of paratyphoids in addition to the true typhoid was sufficient to take it out of the typhoid class, unless the patient was uninoculated, in which case, of course, the typhoid is as true as can be. We always maintained that typhoid in the inoculated would be regarded 'with suspicion' by the medicoes, and here with charming naïveté we have the process disclosed by which the inoculated officially escape and the uninoculated 'get it in the neck.'"

This method of diagnosis well explains the statement of many an invalided "Tommy":—"First they said I had typhoid and then they said I had paratyphoid and then they said I had dysentery (or vice versa); but it *feels the same* all the time!" To the devout Pasteurian, an illness has little connection with symptoms or feelings; its reality consists in the form of a micro-organism seen through a microscope. As the late Mr. Stephen Paget, Hon. Secretary of the Research (Vivisection) Defence Society, wrote to the *Daily Mail* of April 16, 1920:—"The symptoms of paratyphoid have a general likeness to typhoid; but the *germs* are different." This view-point of disease-conditions leads to the extraordinary obsession that, provided a specific nomenclature be avoided, inoculation has gained a triumph, no matter how great may be the sick-rate, or even death-rate. That this criticism is justified may be seen from the same article in the *Daily Mail* by Mr. Paget, who wrote:—"See, in the light of these facts, the infamy of

[1] *The British Medical Journal*, 2nd September, 1916.
[2] November, 1916.

the suggestion that the protective treatment failed at Gallipoli. It gives me pleasure to nail that lie to the counter." The "facts" to provide "light" are given in a quotation from Dr. Charles Searle, of Cambridge, who has stated:—"Before Gallipoli we only inoculated for typhoid, and the result was that out of 100,000 cases of sickness there were only 425 cases of typhoid and 8,103 of paratyphoid. We were under the most appalling conditions; we were on half a pint of water a day. We drank from any pool of muddy water, any filthy stuff so long as it was moist. There is nothing more terrible than thirst, we had no relief; we lived in the trenches. *Every man was sick, and we had something like 50,000 cases of dysentery*; but we had only a very small proportion indeed of typhoid." Dr. Searle continues by giving some figures for Egypt and Palestine in regard to typhoid and paratyphoid, incidentally interjecting that "*there was any amount of dysentery in Palestine.*" All we can say is that the official figures for these countries have been repeatedly asked for in Parliament, and that they have not yet been provided. But to return to Gallipoli, Colonel Martin and Major Upjohn have described the kind of bacteriological diagnosis that brings about the naming of diseases, while Dr. Searle himself bears witness that "every man was sick" and puts forward figures to show that nearly 60,000 were down *with directly intestinal complaints*. Granted that the conditions were "appalling": we are not denying it, though they might, possibly, have been less bad but for the extravagant assurances of the value of preventive inoculation, which inclined those in command to take less precautions about a pure water supply. What we are debating is whether our troops, especially the hardy Anzacs, would not have withstood those conditions very differently had they been free from the pollution of Pasteurian interference. This obsession of viewing disease from the standpoint of micro-organisms, regardless, too, of their possible variability, seems to blind the reason to the obvious fact that in serious illness mere nomenclature can be of no solace to the

patient; neither would it console a mourner to be assured that dysentery rather than typhoid had been responsible for the loss of his or her friend or relative. Of what value is an artificial immunity from a particular complaint, if a similar complaint be its substitute? Upon general health and disease-rate must the matter be judged, and when again we learn from General Smuts, in regard to the East African campaign, that "disease has wrought havoc," we are once more provided with proof of the failure of Pasteurian methods in the Great War.

Another pæan of medical victory that has been sung, even from such an unsuitable vantage-ground as the pulpit of St. Paul's,[1] is that of inoculatory success in regard to tetanus. The prophylactic use of anti-toxin is claimed to have modified the complaint.

What, however, are the proofs of this claim?

When we consider the commencement of the war, we find that Sir David Bruce had stated[2] that the ratio of tetanus in September, 1914, was 16 per 1,000; in October, 32 per 1,000; and in November only 2 per 1,000. Sir David admits "several factors at work in September and October, 1914, to raise the ratio," but for the drop vaunts as "the most important factor—the prophylactic injection of tetanus anti-toxin." "This was not carried out during the first two months of the war," he says, though this assertion is modified by his disclosure of "the amount of serum sent out to France in the first five months—August, 1914, 600 doses; September, 12,000; October, 44,000; November, 112,000; December, 120,000." He refers to a letter from Sir William Leishman, who "feels sure that the drop in the incidence of tetanus in November, 1914, was due to the use of the prophylactic dose, and does not think any large complicating factor comes in." To those who recall the insufficiency of ambulances and medical appliances in the early days of the Great War, an immense complicating factor is self-evident, and this Sir

[1] By Dean Inge.
[2] See *The British Medical Journal,* January 27th, 1917, p. 118.

David Bruce himself acknowledges when he describes "the difficulty of collecting the wounded on account of their numbers and the movement of the troops, and finally the difficulty of giving the thorough surgical treatment to their wounds which is so essential in the fight against tetanus."

In passing judgment upon all preventive treatment there is naturally always an initial difficulty as to whether any given complaint has really been prevented or whether it would not have appeared in any case. In tetanus, this difficulty is augmented by the fact that the anti-tetanic injection, following customary Pasteurian procedure, as in hydrophobia, has brought about the creation of a new disease. *The Lancet* for October 23rd, 1915, refers to Dr. Montais' observations, as set forth in the *Annales de L'Institut Pasteur*:—"Dr. Montais has collected from French sources alone no less than 21 cases of purely local tetanus, without trismus, as well as a number of similar cases in which trismus and other general symptoms later intervened. *All were in persons who had received a prophylactic injection of serum.* Although the form of tetanus which begins locally and is followed by trismus has long been known, pure local tetanus is a pathological novelty in man. The condition, Dr. Montais claims, *is the creation of preventive serotherapy.*" Again, *The Lancet* for January 27th, 1917,[1] contains an article on *Modified Tetanus* by Captain H. Burrows, which begins as follows:—"There are two reasons why the subject of tetanus should be of interest at the present moment. In the first place, the disease still occurs among the wounded. During the months of July, August and September, 1916, at the General Hospital, we had one case of tetanus in every 600 cases of gunshot wound. And this, of course, does not represent the full liability, for cases have occurred in patients who have been evacuated to England, and possibly at casualty clearing stations also. In the second place, a large proportion of the cases which have been seen recently have been ab-

[1] p. 139.

normal in character, inasmuch as the muscular spasms have not become general. They have remained localised to the muscles in the neighbourhood of the original wound. . . . In local or modified tetanus *we have a new form of disease*. The disease is new because its cause is new, for local tetanus is tetanus modified by the prophylactic use of anti-tetanic serum."

We see the inference here that tetanic anti-toxin has mitigated what, without it, would have been definite cases of ordinary tetanus. So in one of the Military Medical Manuals, entitled *Abnormal Forms of Tetanus*, by Courtois-Suffit and R. Giroux, edited by Surgeon-General Sir David Bruce and Frederick Golla, M.B., and published in 1918, we find the opinion that:—"One fact alone tends to emerge, and that is the undoubted effect which anti-toxin given prophylactically has in modifying the disease."

But we want to know how and why? Since this "new disease," local tetanus, is, on the whole, a concomitant of serum treatment, what real ground is there for assuming that it is a mild and safer form of an otherwise virulent and fatal onslaught of tetanus? Can the discharged soldier with a limb contracted for life really take comfort that but for inoculatory measures he would have been a dead man? May he not equally lament that but for serum treatment he might have retained the full use of his members?

The weakness of serotherapy comes out, we consider, when dealing with the factor of time in regard to preventive measures. It has been stated by Sir William Leishman and Major A. B. Smallman[1] that:—"It is, of course, well known that the earlier the preventive dose is taken after the receipt of the wound the more likely it is to be of use"; though with the usual prevarication that invariably safeguards all Pasteurian pretensions, they in the same breath assert:—"At the same time there is little positive information as to the effects of delay." Be this as it may, they go on to describe the conditions that made delay inevitable:—"It should be borne in mind that delay in

[1] *The Lancet*, January 27th, 1917, p. 133.

giving the preventive inoculation is almost always caused by the impossibility of removing the wounded man from the place where he was hit till military conditions permitted. Such cases are therefore specially liable to gangrene and to the more severe forms of septic trouble." This confession turns the search-light of commonsense upon the question. The men who received early doses of serum were the men who were rescued off-hand and whose wounds gained prompt cleansing from filth with its untoward influence upon their muscular and nervous systems. The men who went without or received belated serum treatment were the men whose wounds were left to fester for hours, or even days, the unhappy victims abandoned in shell-holes, or left exposed in No Man's Land to the hell-fire of shell and bullet. Is it not self-evident that these, rather than others, must have fallen victims to tetanus, and that quite apart from any question of inoculation?

What inoculation, however, appears to have done is to have introduced a new form of tetanus which vitiates statistical judgment of the death-rate. We read, for example, in the same Military Medical Manual, *Abnormal Forms of Tetanus*:—"Inasmuch as the true local tetanus has practically no mortality, it may readily be seen how the introduction of such cases in statistics of tetanus has reduced the apparent mortality of the disease, and incidentally encouraged many observers to regard the reduction of mortality as a demonstration of the efficacy of some particular form of treatment."

Leaving the prophylactic and turning to the curative aspect of the anti-tetanic serum, even such orthodox critics as Sir William Leishman and Major Smallman[1] have had to allow that:—"There exist wide differences of view both as to the usefulness of anti-toxin at all, and, admitting its value, as to the system of its employment"; while, in announcing a case-mortality from tetanus of 78.2 per cent in hospitals in France, they have had to

[1] *The Lancet*, January 27th, 1917, p. 131.

admit:—"This, as far as it goes, does not disclose any considerable degree of improvement in the treatment employed." The contradictions as to the different routes of administration throw light upon the experimental nature of the treatment. "Taking the intravenous route first," Leishman and Smallman "are in full agreement with the recommendation of the Tetanus Committee in their revised Memorandum that this route should not be used; not only does it introduce a risk of anaphylactic shock, from which other methods are practically free, but it appears to us from our records that it has done little, if any, good in treatment." "As to the intrathecal route—the study of our own cases has not impressed us favourably . . . the evidence is pretty strongly against its employment . . . the method appears to us to possess some very definite disadvantages and dangers. . . . In at least one case, death followed rapidly upon a thecal dose when the patient was said to have been progressing favourably."

Here we have a specific example of the dangers that our soldiers and sailors had to face from Pasteurian methods as an aftermath of the risks they ran from the Germans, for, in spite of being dubbed "dangerous," this intrathecal route was the one emphatically recommended by the War Office Committee.[1] Its decision was, apparently, based upon Professor Sherrington's exploitation of monkeys, and so another instance is provided of the misleading results of experimentation on live animals. As regards clinical observation of the treatment, Sir David Bruce has supplied a comical instance.[2] Detailing case mortality, with the object of seeing whether "the intrathecal route had any advantage over the other methods of injection,"[3] he proved his highest mortality, 47.1 per cent, to have been in 53 cases treated intrathecally with anti-tetanic serum on the day that the tetanus symptoms declared themselves, and his next highest, 43.7 per cent, in 96 cases treated with the serum also on the same day that the disease set in.

[1] *The British Medical Journal*, July 21st, 1917, p. 89.
[2] *The Lancet*, June 30th, 1917, p. 986. [3] *ibid.* p. 988.

The lowest mortality-rate, 26.0 per cent, was in 23 cases treated with serum, but not intrathecally, on the day after the onset of the disease; while the next lowest, 26.9 per cent, was in 26 cases which received the anti-toxin any time between 3 and 12 days after the appearance of tetanus. Thus Sir David Bruce is driven to bewail:—"Last year (1915-1916) the difference was in favour of the intrathecal route. Now the opposite is true. From these figures it would appear that it is better to defer treatment until the symptoms have been manifest for one or more days. *Quod est absurdum.*" Which commentary on Pasteurian theories and procedure in general may be considered to be a correct pronouncement!

Meanwhile, leaving the doctors to theorise, let us take the figures for tetanus that deal only with the wounded soldiers *that reached the hospitals in the United Kingdom.*

Years	Cases	Deaths
1914.................	192	104
1915.................	134	75
1916.................	501	182
1917.................	353	68
1918.................	266	68

These numbers can surely only be few as compared with the corresponding number in hospitals in the war zones and other quarters. Thus there appears to be no reality in the boast that tetanus was stamped out of the British Army by inoculatory measures. Indeed, it seems the other way about, as we see more clearly by a comparison with two former wars.

If we turn to *The Lancet* for December 29th, 1917, we find *An Analysis of Recent Tetanus Statistics* by F. Golla, M.B., B.Ch. Oxon. In this, while trying to eulogise prophylactic treatment for a lengthening of the incubation period, Captain Golla has to make the following striking admissions in regard to the Franco-Prussian war, where inoculation was unknown, and the recent war with its cult of injections. On page 968, referring to cases of tetanus, we read:—"If, however, the first three week periods are com-

pared it will be found that during the first two weeks the mortality in the 1916 cases is slightly below that of 1870—i.e. 75.5 per cent, and 70 per cent, as against 96.5 per cent and 85.5 per cent—whereas in the third week *the 1916 mortality is slightly above that of 1870.* This is precisely what we should expect on the hypothesis that the slight initial diminution of mortality is due to our improved methods of rendering first aid to the wounded and abstention from drastic operations. After the first two weeks when the cases of exhaustion and post-operative shock become fewer, the mortality from both statistics becomes practically the same in the third week. On the hypothesis that the slight diminution of mortality is due to therapeutic serum treatment alone, there would appear to be no reason to account for serum treatment being less efficacious in the third week than in the two preceding weeks. It must at any rate be conceded that if the slight initial decrease of mortality is all that can be claimed for serum treatment the result is not very encouraging."

Thus a graceful apology is made for a mortality that, in the third week period, actually outnumbers that of a war that took place over fifty years ago.

To come to more recent times, let us quote information supplied by Mr. Churchill in the House of Commons on July 6th, 1920. In reply to a question, he stated that there were only 6 cases of tetanus among the soldiers wounded or injured in action in the South African war; that is, an attack-rate of .28 per thousand. Further, he stated that there were 3 deaths, or a death-rate of .14 per thousand. There were *no* cases of tetanus among officers.

Asked to supply the same information in regard to the late war, Mr. Churchill, two days later, was not able to give any figures except as regards the western front, where he omitted to state the number of cases and deaths. The attack-rate he gave as *approximately* 1.22 per thousand and the death-rate as *approximately* .49 per thousand. We have already seen that the fatality-rate is reduced by the inclusion of local tetanus, which appears to have no mor-

tality; but, even when disregarding this convenient statistical factor, the attack and death-rates remain greater than among the troops in South Africa with whom "preventive" inoculation against tetanus was entirely unknown!

To sum up, medical results throughout the Great War have not equalled in any measure the surgical. This is the more remarkable in view of the modern improved methods of hygiene, the splendid system of nursing and the grand self-sacrifice of most of the Army doctors and nurses. Pasteurian methods alone seem to account for the medical success falling short of the surgical.

As regards this, we may instance the prevalence of sepsis. Even such an orthodox Pasteurian as Dr. Saleeby[1] has admitted that the war "raised new problems, not least in regard to septic wounds, of a number and kind which reach serious military importance and which the previous experience of our surgeons has scarcely encountered."

The trouble was, of course, conveniently ascribed to a malignant organism inhabiting manured soil; but with the tiresome perversity with which Nature knocks over such plausible excuses, wounds received at sea, where there is no soil at all, proved to be as septic as wounds encountered on land. Had our medicoes followed Béchamp's teaching as the Frenchman, Galippe, has done, they would, like him, have understood the phenomena due to microbiosis, the part played by the crushed tissues and the extravasated blood, which, through their inherent microzymas, may give birth, in themselves, according to Galippe, to infectious elements.[2] It seems reasonable to imagine that such trouble would be far more likely to arise in blood contaminated by Pasteurian nostrums than in the unpolluted blood of healthy subjects.

The *Vaccination Inquirer*[3] sums up the matter succinctly:—"It looks more than probable that the doctors have been at their ancient practice of sowing with one hand the disease

[1] See the *Daily Chronicle*, January 18th, 1917.
[2] See Chapter XIV of the present work.
[3] March, 1917, p. 36.

which they pretend to cure with the other, of course in all stupidity and good faith."

It is an unhappy fact that, apart from generalisations, the war has provided concrete instances of the truth of this opinion. We will only refer to one illustrative example, the enforced inoculation of the Bedford Regiment on board *The Empress of Britain* on her voyage from South Africa to India in April of the year 1917. Although the vessel was vermin-ridden and the water supply, both as regards drinking and washing purposes, quite inadequate, the inoculation of the men of the Bedford Regiment was insisted upon, in spite of advice to the contrary. The result was that 10 died on board, 5 more after landing at Bombay; while, in addition, 50 men were laid low with serious illness. And actually, no official inquiry has ever taken place in regard to this highly regrettable incident, such is the smoke-screen that shields even the most flagrant Pasteurian perpetrations.

But, all the same, there is an evidence that no Government can blot out—the evidence of the facts of life, the danger-signals of experience. Though the careless may pass them by, to the observant they stand out in warning, like the ominous hand that startled the roysterers in ancient Babylon, and, happily, there are still Daniels in our midst with the gift of interpretation. However, we must leave "the writing on the wall" for consideration in the next chapter.

CHAPTER XX

The Writing on the Wall

The whole subject of injecting into the body foreign matters associated with disease-conditions must be considered broadly from every aspect. Perhaps no better opinion can be quoted than that of the great thinker, Herbert Spencer, for what applies to one injection must also have some application to all others.

In the chapter on vaccination, in his book *Facts and Comments*, the philosopher quotes the following remark of a distinguished biologist:—"When once you interfere with the order of Nature there is no knowing where the results will end." Mr. Spencer continues:—"Jenner and his disciples have assumed that when vaccine virus has passed through a patient's system he is safe, or comparatively safe, against smallpox, and that there the matter ends. I will not say anything for or against this assumption." We may add that he does, however, in a foot-note, which is decidedly *against*. Then he proceeds:—"I merely propose to show that there the matter does *not* end. The interference with the order of Nature has various sequences other than that counted upon. Some have been made known."

"A Parliamentary Return issued in 1880 (No. 392) shows that comparing the quinquennial periods 1847-1851 and 1874-1878 there was in the latter a diminution in the deaths from all causes of infants, under one year old, of 6,600 per million births per annum; while the mortality caused by eight specified diseases, either directly communicable or exacerbated by the effects of vaccination, increased from 20,524 to 41,353 per million births per annum—more than double. It is clear that far more were killed by these other diseases than were saved from smallpox."

Again comes a foot-note, which is worth quoting.

"This was in the days of arm-to-arm vaccination, when medical men were certain that other diseases (syphilis, for instance), could not be communicated through the vaccine virus. Anyone who looks into the Transactions of the Epidemiological Society of some thirty years ago, will find that they were suddenly convinced to the contrary by a dreadful case of wholesale syphilisation. In these days of calf-lymph vaccination, such dangers are excluded; not that of bovine tuberculosis, however. But I name the fact as showing what amount of faith is to be placed in medical opinion."

Once more he continues:—"To the communication of diseases thus demonstrated, must be added accompanying effects. It is held that the immunity produced by vaccination implies some change in the components of the body; a necessary assumption. But now if the substances composing the body, solid or liquid or both, have been so modified as to leave them no longer liable to smallpox, is the modification otherwise inoperative? Will anyone dare to say it produces no further effect than that of shielding the patient from a particular disease? You cannot change the constitution in relation to one invading agent and leave it unchanged in regard to all other invading agents."

We may here interpolate that how much more must this be the case if disease-conditions depend upon inherent organisms.

"What must the change be?" inquires Mr. Spencer.

"We have no means," he says, "of measuring alterations in resisting power, and hence they commonly pass unremarked. There are, however, evidences of a general relative debility. Measles is a severer disease than it used to be, and deaths from it are very numerous. Influenza yields proof. Sixty years ago, when at long intervals an epidemic occurred, it seized but few, was not severe, and left no serious *sequelæ*, now it is permanently established, affects multitudes in extreme forms, and often leaves

damaged constitutions. The disease is the same; but there is less ability to withstand it.

"There are other significant facts. It is a familiar biological truth that the organs of sense and the teeth arise out of the dermal layer of the embryo. Hence abnormalities affect all of them; blue-eyed cats are deaf, and hairless dogs have imperfect teeth (*Origin of Species*, Chapter 1). The like holds of constitutional abnormalities caused by disease. Syphilis in its earlier stages is a skin-disease. When it is inherited the effects are malformation of teeth and in later years iritis (inflammation of the iris). Kindred relations hold with other skin-diseases: instance the fact that scarlet-fever is often accompanied by loosening of the teeth, and the fact that with measles often go disorders, sometimes temporary, sometimes permanent, of both eyes and ears. May it not be thus with another skin-disease—that which vaccination gives? If so, we have an explanation of the frightful degeneracy of teeth among young people in recent times; and we need not wonder at the prevalence of weak and defective eyes among them. Be these suggestions true or not, one thing is certain: The assumption that vaccination changes the constitution in relation to smallpox and does not otherwise change it is sheer folly."

"Is it changed for the better?" finally questions Mr. Spencer. "If not, it must be changed for the worse."

The great thinker and observer delivered this warning against only one form of injection. How much greater must be the danger in view of the myriad and frequent inoculations in fashion at the present day? We are reminded of an invalided Australian soldier in the medical ward of a London Hospital, who, upon being asked whether he believed in inoculation, replied:—"Well, hardly! I've been inoculated against half a dozen complaints, and I've had everything I've been inoculated against except cholera, and I daresay I'll be getting that yet!"

"All is danger," wrote Béchamp[1] long ago, "in this kind of experimentation, for the reason that it is not anything inert that is acted upon but that there is a modification, more or less injurious, of the microzymas of the inoculated."

Many long years after this statement of his opinion, a remarkable confirmation has been given by outbreaks of a disease of the central nervous system, commonly known as encephalitis, and which has so often followed vaccination that compulsory vaccination has been suspended in Holland, and its abolition suggested by a medical congress in Sweden; while, even in Germany, its dangerous possibilities have been officially recognised.

The cases of post-vaccinal-encephalitis in England resulted in the appointment of two Committees of Investigation, whose Reports, published in July, 1928, dealt with 90 cases, 52 of which ended in death. In answer to a question in Parliament, on February 26th, 1932, the Minister of Health gave the latest figures as 197 cases, with 102 deaths.

As a consequence of this serious development, the Ministry of Health, in August, 1929, issued a new Vaccination Order reducing the vaccination marks from 4 to 1, and, in the accompanying circular, referring to this danger, suggested that it was inexpedient to vaccinate for the first time adolescents or children of school-age. Controversy continues as to the cause of the malady, Professor James McIntosh, of London University and the Middlesex Hospital, attributing it to the actual vaccine, while other investigators consider that this simply arouses some existent but hitherto latent trouble.

During the very period in which sanitation and hygiene have played a part unknown before in recorded history, a disappointing deterioration seems discernible in the human physique. The fashion for crowding to cities, the strain of the wear and tear of modern existence, and the breeding of the unfit are, no doubt, numbered among

[1] *Les Microzymas*, p. 902.

contributory causes, which, however, cannot omit human experimentation, for nothing short of this is the introduction into bodies of poisons whose far-reaching effects are entirely beyond knowledge and control.

On the face of it, how futile to attempt individual safeguards against a disease like smallpox, which can only be eliminated in the mass by general cleanliness, while gruesome dangers, such as cancer, exhibit a hideous warning against playing with unknown quantities. We do not attempt to theorise upon the causation of malignant growths, but we would certainly point to their alarming increase. According to a statement put forward on the authority of the Cancer Research Fund, one man in twelve and one woman in eight over forty years of age are liable to this horrible torment. In regard to the useless, mis-directed efforts made against it, F. E. R. McDonagh, L.R.C.S., in "*The Nature of Disease Journal*," Vol. 1 (1932) writes:—"Over £4,000,000 have been wasted upon cancer research." For the 10 years 1922-31, there were over 180,000 experiments on animals. In some cases, a single one of these experiments may have involved the sacrifice of from 40 to 50 creatures. The complete non-success of these vivisectional cruelties is well evidenced by the steady increase in the disease shown by the statistics of the Registrar-General.

Deaths from Cancer in England and Wales

1891-1900=23,218	Yearly Average	1921=46,022
1901-1910=30,914		1922=46,903
1912=37,323		1923=48,668
1913=38,939		1924=50,389
1914=39,517		1925=51,939
1915=39,847		1926=53,220
1916=40,630		1927=54,078
1917=41,158		1928=56,253
1918=41,227		1929=56,896
1919=42,144		1930=57,883
1920=42,687		1931=59,346

The number of Deaths each year from Cancer out of every Million Persons Living was as follows:—

1891-1900=758 } Yearly	1921=1,215
1901-1910=900 } Average	1922=1,229
1912=1,021	1923=1,267
1913=1,055	1924=1,297
1914=1,069	1925=1,336
1915=1,121	1926=1,362
1916=1,166	1927=1,376
1917=1,210	1928=1,425
1918=1,218	1929=1,437
1919=1,145	1930=1,454
1920=1,166	1931=1,484

When such ominous danger-signals flare into view after a century of vaccination, the thoughtful may well contemplate with alarm the risks of the modern fashion for wholesale inoculation. That medical orthodoxy should be blind to Pasteurian dangers will not surprise the student of medical history. He has, for instance, only to remind himself how, in 1754, the Royal College of Physicians in a formal Minute pronounced the inoculation of smallpox to be "highly salutary," and how in 1807, the same body, in reply to a question from the House of Commons, declared it to be "mischievous." Fashions in medicine, like fashions in clothes, change from generation to generation, and it is as difficult for a medical man to break away from the one as it is for a society *belle* to free herself from the trammels of the other. Independence of income, as well as independence of intellect, is needed for a man to set aside teaching received, not as theory, but as dogma, and at a most impressionable age; for, if the desired goal be the attainment of worldly ambition, unquestioning orthodoxy is the price that has to be paid. So long as the discovery of a "microbe" may assist to a medical knighthood and the discovery of a "vaccine" to a comfortable income, no one need be surprised at the popularity of the theory of causative disease-germs with its consequent system of inoculations.

The dangers of Pasteurism, moreover, have never been

revealed in the light of Béchamp's doctrine that "the microzyma is at the beginning of all organisation," and that "every organism may be reduced to the microzyma." Thus, if he be correct, our corporate life is composed of a united multiplicity of infinitesimal cytological and histological elements, each possessed of its own independent being. According to Béchamp, it is because every organism is reducible to the microzyma that life exists in the germ before it develops organs. It is because there are in the microzyma permanent principles of reaction that we have at last realised some idea of life. It is because the microzymas are endowed with an individual independent life that there are in the different centres of the body differing microzymas, with varying functions. This biological teaching throws light on the potency of the minutely delicate homeopathic dosage; it explains the changes that must be involved by what Herbert Spencer called "invading agents," a danger immediately sensed by his genius, quite apart from such teaching as that provided by Béchamp in whose great work *Les Microzymas*[1] we find the following passage:—

"The most serious, even fatal, disorders may be provoked by the injection of living organisms into the blood; organisms which, existing in the organs proper to them, fulfil necessary and beneficial functions—chemical and physiological—but injected into the blood, into a medium not intended for them, provoke redoubtable manifestations of the gravest morbid phenomena. . . . Microzymas, morphologically identical, may differ functionally, and those proper to one species or to one centre of activity cannot be introduced into an animal of another species, nor even into another centre of activity in the same animal without serious danger."

How much more dangerous is it then, when the microzymas, artificially inoculated, are not only of a foreign species, but are in a morbid condition, even in the species from which they are taken.

[1] *Les Microzymas*, p. 690.

Béchamp follows up the passage, above quoted, with a description, based upon experiments, of the microzymian capacity for changing function. It would seem that Pasteurians, in their fear of parasites, have overlooked the effects of inherent elements and have reduced their system of inoculation to one of raw experimentation. Already they appear to have commenced a retreat from their boosted vantage ground. We refer, for instance, to the views of Dr. Besredka of the Pasteur Institute, which the *British Medical Journal* has described as "subversive of the ideas hitherto held by bacteriologists." *The Times* of the 28th August, 1920, sums up Besredka's teaching as follows:—"Here, then, was the idea that immunity or protection against dysentery is not an affair of the blood at all, but an affair of those special parts of the body in which the dysentery germs live and act. In short, that salvation is not by antidote, but by some local effect, 'the intestinal barrier becomes unbreakable,' whatever the nature of the barrier may be. This, it will be seen, is a conception of an absolutely different kind from that to which we are accustomed. One result—for the work applies also to typhoid fever—is that vaccination as now practised is unnecessary." Thus away overboard goes the whole Pasteurian theory of immunity and with it the system of inoculation; for, according to Dr. Besredka:—"vaccination is only efficacious when the vaccine finally reaches the intestine or certain zones of it. . . . The mode of vaccination to be preferred is the oral route."

The Times, of the 31st August, 1920, further comments:—"These results turn attention positively from the seed to the soil, from germs to the men and animals who may harbour them." And in so doing the advice is followed that was given so long ago by the great doctor, Professor Antoine Béchamp.

So much for the shufflings of those who have based their work on the teachings of Louis Pasteur; and we cannot but commiserate the innocent among the public who blindly submit their bodies to the shifting fashions of Pasteurian

treatment. The victimising of animals has brought about its logical sequence—the victimising of human beings! For this, we have to thank the imitator of Edward Jenner, the chemist, Louis Pasteur, who, by a majority vote of one, gained his place among the Free Associates of the Academy of Medicine. Thus has the most jealous trade-union in the world, that of the orthodox Medical Faculty, been completely brought under the sway of an outsider with no pretensions to being a physician!

VALEDICTORY

CHAPTER XXI

Pasteur and Béchamp

On an autumn day, in the year 1895, the normal life of Paris gave way to the pageantry of a pompous funeral. The President of the French Republic, Members of Parliament, Government officials, Members of Scientific Societies thronged to the obsequies of their compatriot, Pasteur, whose world-wide fame seemed to do honour to all France. In life, in death, no scientist ever reaped so great a meed of glory.

Symbol of worldly prosperity, in the centre of the Pasteur Institute, is the costly chapel, resplendent with marble, porphyry and lapis lazuli, where the poor paralysed body has crumbled to dust beneath recorded boasts that read very strangely to those who have delved into the old scientific records of the period. Here, for instance, on the walls of the chapel, we find inscribed:—

"1857." "Fermentations."
"1862." "So-called Spontaneous Generation."
"1863." "Studies in Wine."
"1865." "Diseases of Silk-Worms."
"1871." "Studies in Beer."
"1877." "Virulent Microbic Diseases."
"1880." "Vaccinating Viruses."
"1885." "Prophylaxis of Rabies."

Now, what as to these vaunted triumphs?

"1857." "Fermentations."

The *Encyclopædia Britannica* tells us that Pasteur's "theory of fermentation was materially modified. . . ." And this, as we have seen, was inevitable in consequence of his

separating this chemical phenomenon "from the acts of ordinary life," and in so doing proving that he did not understand Béchamp's explanation of fermentation as the result of acts of assimilation and excretion.

"1862." "So-called Spontaneous Generation."

We have seen that Pasteur never satisfied the Sponteparists, and that his very experiments sometimes contradicted his own conclusions.

"1863." "Studies in Wine."

In dedicating his work to Napoleon III, Pasteur wrote:—"Sire,—If, as I hope, time consecrates the exactness of my work. . . ."

Dr. Lutaud comments[1]:—"The hope has been misplaced. Time has not consecrated the exactness of this work. All who placed confidence in this process underwent heavy loss. Only the State persisted in heating the wines destined for the armies of land and sea. This rendered them so bad that the men preferred to drink water. It is high time that the apparatus for heating wines—according to the Pasteur system—should be put into the melting-pot."

"1865." "Diseases of Silk-Worms."

We have seen how, in regard to these complaints, Béchamp provided Pasteur with the correct diagnosis, and that after the latter inaugurated his system of grainage, "the salvation of sericulture" was a drop in production, according to M. de Masquard, from 15,000,000 to 8,000,000 and, later on, to 2,000,000 kilogrammes.

"1871." "Studies in Beer."

Dr. Lutaud[2] tells us that the boast that French breweries owe an incalculable debt to Pasteur is best answered by the facts that the latter's process was abandoned as impracticable and that the brewing of beer in France is

[1] *Études sur la Rage et la Méthode Pasteur*, p. 429.
[2] *Études sur la Rage*, pp. 428, 429.

almost nil, most of the amount found there having been imported from Germany.

"1877." "Virulent Microbic Diseases."

We have seen how Pasteur opposed the microzymian doctrine after failing in an apparent discreet attempt at plagiarism, and followed instead the ideas of Linné, Kircher and Raspail.

"1880." "Vaccinating Viruses."

The Sanitary Commission of the Hungarian Government in 1881, thus reported upon the anti-anthrax inoculation:—"The worst diseases, pneumonia, catarrhal fever, etc., have exclusively struck down the animals subjected to injection. It follows from this that the Pasteur inoculation tends to accelerate the action of certain latent diseases and to hasten the mortal issue of other grave affections."

As we have said, the Hungarian Government forbade the use of the inoculations.

"1885." "Prophylaxis of Rabies."

Dr. Lutaud[1] reminds us how Professor Peter put pertinent questions to the Academy of Medicine on the 18th January, 1886, in the early days of Pasteur's so-called preventive treatment.

"Has the annual mortality from hydrophobia in France been diminished by the anti-rabic medication?"

"No."

"Does this mortality tend to augment with the intensive rabic methods?"

"Yes."

"Where then is the benefit?"

As we have seen, the benefit lies in the monetary returns gained by makers of such nostrums. Pasteurism has become a vested interest, and one, unfortunately, supported by that powerful trade-union—the Medical Fraternity.

Far be it from us to deny that Pasteur's place in the

[1] *Études sur la Rage*, p. 404.

world of science was gained by genius, the genius for business, and he was certainly not of the order of intellectuals who disregard the allurement of L. S. D. Although he professed reverence for religion, we find, on the authority of Dr. Lutaud,[1] that he secured the election to the Institute of the physiologist, Paul Bert, who had been objected to as an atheist. Dr. Lutaud claims that he did not scruple, moreover, to bring about this election at the expense of his old friend and benefactor, Davaine, and made a condition of it that Bert, a Member of the Budget Commission and all-powerful with the Government, should obtain for him a pension of 25,000 francs.

We, whose lot is cast in an age of advertisement, can appreciate Pasteur's power in this direction. Never has anyone lived who was a greater adept in pushing forward himself and his theories. Ambition was his driving-power, which an iron will held in harness. Before any triumph had met him, his mind was set upon honour and glory. Early in his married life, when according to his biographer,[2] "success did not come," Mme. Pasteur wrote to her father-in-law:—"Louis is rather too preoccupied with his experiments; you know that those he is undertaking this year will give us, if they succeed, a Newton or Galileo." The admiring wife was unaware of her testimony to her husband's self-interest. There is no allusion to any excitement as to the secrets that Nature might unfold. The exaltation of the individual is made the pivot of hope. More than this, as we study his life, we find, throughout, his cleverness in allowing others to sound his praises, while, at the same time, he himself gave vent to self-depreciation; he thus, apparently, garmented himself in a humility seemingly not quite sincere, when we take note of his indignation against those, like Béchamp, who in asserting their just claims, in any way detracted from his own honour.

On no account would we deny his power in gaining

[1] *Études sur la Rage*, pp. 409, 410.
[2] *The Life of Pasteur*, by René Vallery-Radot, p. 78.

affection. Parents, sisters, wife and children all appear to have lavished love upon him; while he also seems to have held the devotion of those who worked for and with him, and, on his side, to have been as good a friend to those as he was a bitter antagonist to all who differed from him.

The claim of a tender heart has been advanced by his admirers. In his biography[1] we read:—"He could assist without too much effort, writes M. Roux, at a simple operation such as a subcutaneous inoculation, and even then, if the animal screamed at all, Pasteur was immediately filled with compassion, and tried to comfort and encourage the victim in a way which would have seemed ludicrous if it had not been touching."

Such a comment certainly shows that M. Roux was himself too devoid of sensibility to be a fit judge of it.

He goes on to describe the first trephining of a dog for Pasteur's benefit, and winds up:—"Pasteur was infinitely grateful to this dog for having borne trephining so well, thus lessening his scruples for future trephining."

So the gradual hardening process went on until any original compunction was blunted, leaving Pasteur unimaginatively callous to the sufferings he caused. An example may be taken from the journal *L'Illustration*:—[2]

"The inoculated dogs are shut in circular cages, provided with a solid close network. It is one of these dogs, in the paroxysm of rabies, which M. Pasteur showed us, observing:—'He will die to-morrow.' The animal looked at him, ready to bite. M. Pasteur having kicked the wires of the cage, the animal dashed at him. It bit the bars, which became red with bloody saliva. Then, with its jaws bleeding, it turned, tearing the straw of its litter, back into its kennel, which it had gnawed the preceding night. From time to time it uttered a piercing and plaintive cry."

This teasing, worrying kick at the bars of the cage of his piteous victim, a dog, that true friend of man, ready to lay

[1] *The Life of Pasteur*, by René Vallery-Radot, p. 318.
[2] May 31st, 1884.

down his life in his service, is the best commentary upon the heart of Louis Pasteur. Tenderness may have been for him all right in its place, but it was quite out of place when it stood in the way of ambition. Personal success dominated all other considerations, and the attainment of this was made easy by a forcefulness and tenacity nothing short of remarkable. Such traits are seen everywhere to be more cogent factors of worldly success than high intellectual ability. Of the latter his childhood gave little evidence. His son-in-law honestly tells us[1]:—"Those who would decorate the early years of Louis Pasteur with wonderful legends would be disappointed: when he attended the daily classes at the Arbois College he belonged merely to the category of good average pupils."

His strongest force was his will-power, of which he wrote to his family[2]:—"To *will* is a great thing, dear sisters, for action and work usually follow, and almost always work is accompanied by success."

Here again, as ever, we find success the leading motive of his life. Had he not put personal ambition before love of science, it would seem impossible for him to have opposed the fellow-worker whose ideas, in numbers of instances, he unquestionably pirated. Had his forcefulness and great business ability been harnessed to Béchamp's idealistic intellect and all-round knowledge, signal services might have been rendered to science, which now, on the contrary, students of its history feel that Pasteur has often led to wrong issues, so that years have been wasted over unsatisfactory theories at the cost of vast animal suffering and a dangerous form of experimentation on human beings. Time has, indeed, brought him triumph in the shape of worldly acclamation. This is hardly surprising, for the way of popularity is through the wide gate, easy of entrance. Pasteur, during his life, despised and detested by a few keen-sighted observers who saw through his pretences, was in general a popular man,

[1] *The Life of Pasteur*, p. 7.
[2] *The Life of Pasteur*, by René Vallery-Radot, p. 15.

and his cult of the microbe is a popular theory, which the least scientific can easily understand: riches and prosperity attend upon it, as glory and renown attended upon him. Why should the ambitious imitate the self-immolation of the truth seeker, Béchamp, who in his lonely apartment, passed away almost unrecognised?

Truth, not self, was Béchamp's lodestar. Like Galileo, the simplest observation led him to his great discoveries, and like Galileo, incessant persecutions, clerical and scientific, pursued him with unrelenting malignity. It was from no lack of hatred in his opponents that he escaped the fate of Servetus, and his great work, *Les Microzymas*, an inclusion in the Roman *Index*.

Never had Truth a more zealous votary than the man who, with Professor Estor, stood quivering with awe-struck amazement at the unfolding of Nature's secrets, self entirely obliterated, every brain-cell concentrated upon astounding revelations. With his extraordinary powers of labour, he amply justified Carlyle's definition of genius—"the capacity for taking infinite pains"; while, also, he absolutely exemplified the reverse side of abnormal faculties, which may be described as —"the capacity for doing with infinite ease that over which others require to take infinite pains." From his boyhood, ordinary studies were to him the lightest of labour, while for his incessant researches no toil was too insistent, no sacrifice too great.

Altogether, he stood on an ethical plane elevated above his fellows. He lived at the same epoch as Pasteur, surrounded by the same callous experimenters, men such as Claude Bernard, whose own daughters felt compelled to forsake him and undertake animal rescue work as some sort of atonement for their father's vivisectional atrocities. Yet Professor Béchamp, as ardent a devotee as ever worshipped at the altar of knowledge, stands out in marked contrast, innocent of cruelty, convicted of pity. In his own multifarious experiments, we come upon no record of brutality, and in reference to Magendie's work, he does

not fail to voice sympathy for "*la pauvre bête*," Magendie's miserable victim. The fact of Béchamp having delved so much deeper into knowledge than his callous contemporaries may well be an instance of the advantage of not blunting a scientific mind by familiarity with cruelty. His imagination possessed to the end the pristine sensitiveness essential to the discoverer, and, spurred and stimulated by his wonderful health and vitality, age itself had no power to dull his intellect.

Devoid of personal ambition, but filled with a passionate yearning for Truth for its own sake, there is no reason to wonder that in astuteness he failed to compare with Pasteur; nor that the crude theory of the latter should have displaced deeper, more complex teaching, which could not in the same way become the immediate property of "the man in the street." The one who might have worked with Béchamp, on the contrary, plagiarised and distorted his ideas. But if we thus seek to dethrone the Idol of Orthodoxy, whom France and the world have delighted to honour, it is only to install another Frenchman as worthy to be ranked among the earth's great luminaries. Like many another of these, it was his fate to meet with neglect and disparagement. Pursued, on the one side, by the jealousy of his less gifted but more successful rival, and, on the other, by narrow-minded men with no understanding of how the Creator can best be interpreted by His Creation, persecution and bitterness of spirit were the earthly rewards of his long life.

Truth is a weary height to scale, its towering pinnacle evasively distant, while climbers are confronted by ever-recurring peaks of difficulty. To make the ascent at all, impedimenta of gain and popularity must be dispensed with, and the adventurous are apt to pass, like Béchamp, out of sight of their contemporaries. What wonder that most, like Pasteur, elect to remain comfortably below in full view! Yet, as we gratefully realise, from time to time Béchamps do arise, lured by the towering mountain-peak, for, were it not so, mankind would forever stagnate at the

same level. And though the leader may never be recognised, the progress of after-centuries may testify unknowingly to his leadership.

Pasteur made a wise remark when he called upon the verdict of time to pass sentence on a scientist. As a matter of fact, Béchamp, with the assurance of genius, never lost hope in this final judgment. The *Moniteur Scientifique* tells us:—"Those of his acquaintance who cared for him and were about him know that he never doubted that one day justice would be rendered him."

It is in this belief, and with this hope, that we have brought forward the story of a great plagiarism and have tried to show the contrast between a successful world-idol and an ignored genius to whom scientists, all unaware, are already indebted for much of their knowledge. Existent, even though often latent, is the sense of fair play and justice in most of us. In this faith, we are emboldened to submit to the Tribunal of Public Opinion the claims of Pierre Jacques Antoine Béchamp, which are embodied in this Lost Chapter of the History of Biology.

THE END

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SOME OF THE WORKS

by

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